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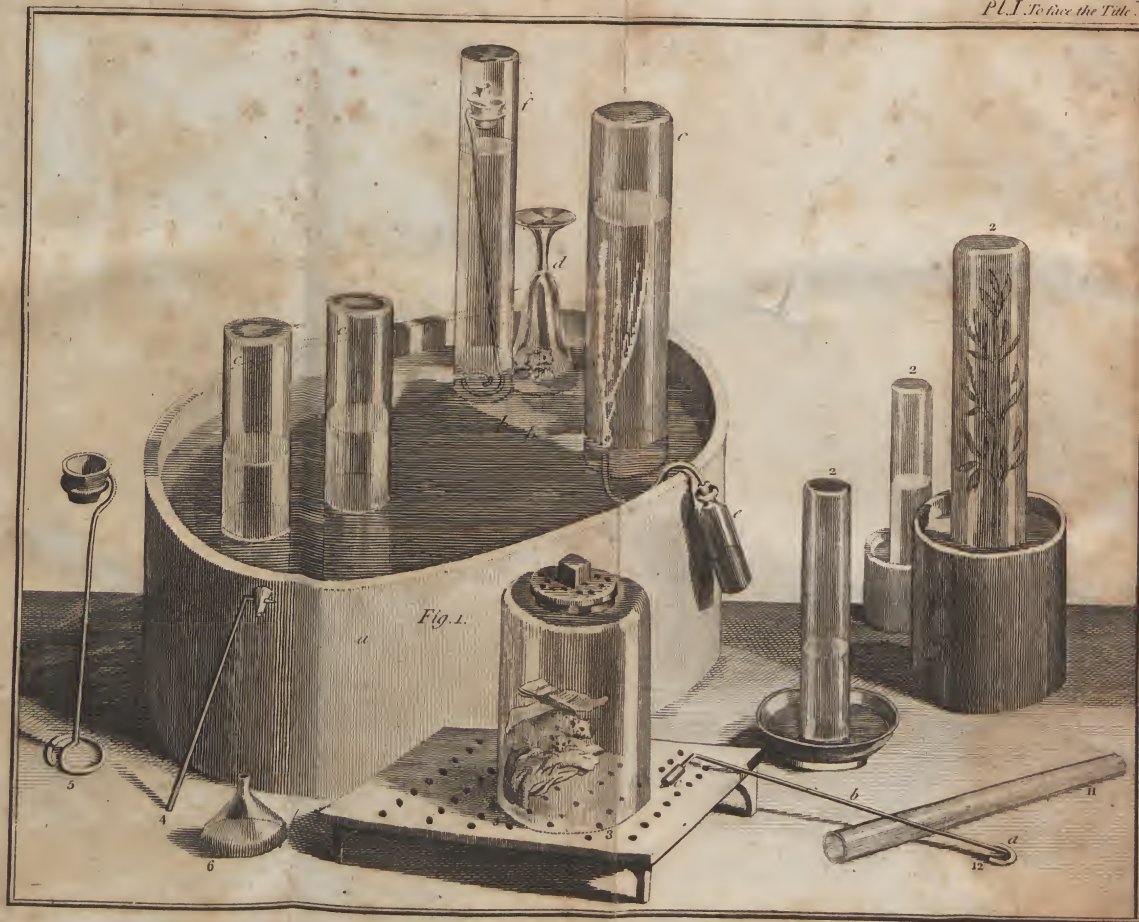


EXPERIMENTS
AND
OBSERVATIONS
ON DIFFERENT KINDS OF
A I R, &c.

QUAMOBREM, si qua est erga Creatorem humilitas, si qua operum ejus reverentia et magnificatio, si qua charitas in homines, si erga necessitates et ærumnas humanas relevandas studium, si quis amor veritatis in naturalibus, et odium tenebrarum, et intellectus purificandi desiderium ; orandi sunt homines iterum, atque iterum, ut, missis philosophiis istis volaticis et preposteris, quæ theses hypothesibus anteposuerunt, et experientiam captivam duxerunt, atque de operibus dei triumpharunt, summisse, et cum veneratione quadam, ad volumen creaturarum evolendum accedant ; atque in eo moram faciant, meditentur, et ab opinionibus abluti et mundi, caste et integre versentur.—In interpretatione ejus eruenda nulli operæ parcant, sed strenue procedant, persistant, immoriantur.

LORD BACON IN INSTAURATIONE MAGNA.





EXPERIMENTS
AND
OBSERVATIONS
ON DIFFERENT KINDS OF
AIR,
AND OTHER BRANCHES OF
NATURAL PHILOSOPHY,
CONNECTED WITH THE SUBJECT.

IN THREE VOLUMES;
Being the former Six Volumes abridged and methodized, with many
Additions.

By JOSEPH PRIESTLEY, LL.D. F.R.S.

AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. ITAL. HARLEM. AUREL.
MED. PARIS. CANTAB. AMERIC. ET PHILAD. SOCIUS.

V O L. I.

Fert animus causas tantarum expromere rerum,
Immenſumque aperitur opus.

LUCAN.

Motto to the Firſt of the Six Volumes.

BIRMINGHAM,
PRINTED BY THOMAS PEARSON;
AND SOLD BY J. JOHNSON, ST. PAUL'S CHURCH-YARD, LONDON.
MDCCXC.

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TO

HIS ROYAL HIGHNESS
GEORGE PRINCE OF WALES,

SIR,

I N dedicating this work to your ROYAL HIGHNESS, I express my own earnest wish, and that of many others, that to your other excellent qualities your ROYAL HIGHNESS may add a disposition to patronize a branch of science, in the extension of which the natives of Great Britain have ever borne a distinguished part, and which has for its object the benefit of all mankind.

It is by increasing our knowledge of *nature*, and by this alone, that we acquire the great art of commanding it, of availing ourselves of its powers, and applying them to our own purposes; true *science* being the

only foundation of all those *arts* of life, whether relating to peace or war, which distinguish *civilized* nations from those which we term *barbarous*; a distinction not less conspicuous than that between some nations of *men* and some species of *brutes*. And that branch of this great science to which the subject of this work relates, viz. *chemistry*, is perhaps of more various and extensive use, than any other part of natural knowledge; and by the application that is now given to it, it is continually growing in relative magnitude and importance.

In the age of Newton chemistry was but little cultivated; and its value not being generally known, it was not regularly taught in places of liberal education, in which *natural philosophy* was always more or less attended to; whereas at present every thing that is not denominated chemistry is but a small part of a system of *natural knowledge*. It is no less remarkable that the doctrine of *air*, of which little or nothing was known in the time of Newton, and which a few
years

years ago was hardly mentioned in the writings of chemists, now makes a very considerable figure in the mass of chemical knowledge, and throws the greatest light on the most important processes.

It is, therefore, earnestly to be wished, that this branch of natural science should be assiduously cultivated; and the patronage of Princes may be eminently useful to this end, by diffusing a taste for it among those whose opulence will enable them to prosecute it to the most advantage.

It is true that we are indebted to the poverty of many persons for some of the most simple and effectual modes of operating in chemistry; *necessity* having in this, as well as in many other cases, been the happy *mother of invention*. But in some cases it is well known that the most promising projects have become abortive for want of the means that were necessary to carry them into execution. For in this science mere *observation* and *reflection* will

not carry a man far. He will frequently have occasion to put the substances which he examines into various new situations, and observe the result of circumstances, which, without *expence*, as well as *labour*, he can have no opportunity of knowing.

Hence it is that the greatest and happiest effects may be expected from the patronage of science by persons of your ROYAL HIGHNESS's rank and expectations, whose wishes and inclinations are often alone sufficient to give a turn to the taste and pursuits of the rich and great. And hitherto almost every country in Europe can boast of more persons among their nobility, and men of fortune, who are devoted to scientific pursuits, than Great Britain.

It will perhaps be said, that men of high rank and fortune in this country are occupied about the greater objects of *civil policy*, and attending to the interests and liberties of the nation. But admitting this to be the case of *all*, which is evidently that of a

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small

small number only, no one object wholly engages the attention of any man. All men have their *pleasures* as well as their *business*; nor is it desirable that any one object should so much ingross any person, as that he should give no degree of attention to any other; and no pursuit can have a juster claim to the leisure hours of men of rank and fortune than that of *natural science*; since, independently of any views of *utility*, none can furnish more rational *amusement*.

Permit us, then, who are engaged in the quiet pursuits of philosophy, to flatter ourselves that they will have the additional recommendation of so effectual a patronage as that of your ROYAL HIGHNESS; and I am persuaded that your ROYAL HIGHNESS does not need to be reminded, that the greatest princes have been the protectors of science and of letters, and that they have ever considered this patronage as reflecting lustre on their crowns.

In

In some countries the sciences seem to require the support of princes, or of the community, by pensions and establishments. In ours these aids are unnecessary. Our *Royal Society*, which gives none but honorary rewards, is all that is wanted in the way of *establishment*; and it has been, and is, eminently useful. In this country patronage is not wanted for those who cultivate the sciences, but rather for the sciences themselves; to give them their due value and consideration, to apply the influence which the great possess over the minds and opinions of men, in directing their tastes to useful pursuits, and thus to incite a sufficient number of able inquirers to explore the hidden powers which the Deity has impressed on matter.

Considering your ROYAL HIGHNESS as destined to be the future sovereign of this country, I cannot wish you greater glory or happiness, than that you should consider it as consisting, not in the *extent*, but in the *flourish*-

flourishing state, of your dominions, to which *science, manufactures, and commerce* (each the true source of the other) will most eminently contribute; and that you should not be dazzled by the flattering, but often fatal, idea of extending what is called the *royal prerogative*; but rather study to give your subjects every power which they can exercise for their own advantage. And whatever flatterers may suggest, the people (each of them giving his whole attention to those things in which he is most interested) will always be able to do more for themselves than the most enlightened and best disposed princes can do for them.

As a person whose deliberate judgment has led him to dissent from the mode of religion by law established in this country, permit me, Sir, to express something more than a wish, that, as the future sovereign of Great Britain you will be the equal father of all your subjects; and that in your reign every man will meet with encouragement and favour in proportion to the services he renders

renders his country, and the credit he is to it.

There has of late years been a wonderful concurrence of circumstances tending to expand the human mind, to shew the inconvenience attending all *establishments*, civil or religious, formed in times of ignorance, and to urge the reformation of them. Let these be suffered to operate without obstruction ; and have the true magnanimity to let no impediment be thrown in the way of the efforts of the more enlightened part of the community to improve the state of it in any respect.

A sovereign conducting himself by these liberal maxims will rank among the few truly *great and good princes*, whose object has not been themselves, and their personal glory and power, but the real good of their country ; and not *that* only, or exclusively, but the benefit of all the human race. A character thus supported will be admired, and beloved, when that of other princes, generally,

generally, but falsely, called *great*, will be
consigned to what is worse than oblivion,
the detestation of all good men.

That your ROYAL HIGHNESS may prove
a truly patriot king, an ornament to human
nature, and a blessing to your country, and
to mankind, is the sincere wish, and prayer,
of

Your ROYAL HIGHNESS's,

Most obedient

And most humble servant,

J. PRIESTLEY.

BIRMINGHAM, }
March 24, 1790. }



T H E
P R E F A C E*.

HAVING, at different times, published six volumes of observations and experiments relating chiefly to the subject of *air*, and they being at present so far out of print, that a complete set cannot be had new, it seemed more advisable to *new model* the whole work, than *reprint* the former volumes.

In such a multiplicity of observations, made at very different times, it could not be but that many must now be superfluous ; and there must also be a variety of imperfections, with which it is not worth while to trouble the reader. It will also be more agreeable to any person who is entering upon these inquiries, to get acquainted with what I have done in a better method than that in which the particulars happened to occur to myself, and especially to

* Into this Preface I have introduced every thing that I thought worth preserving in the prefaces to all the six volumes ; and it is hoped that the importance of the observations it contains, will be a sufficient apology for the length of it.

see all that has been discovered with respect to any subject of experiment, such as any of the different kinds of air, &c. with as little mixture of other matter as possible.

Having had a view to such readers, I have endeavoured in this new edition to digest the contents of all the six volumes, and also of those papers which, since the publication of them, have been inserted in the Philosophical Transactions, into something like a *system*; some regard, at the same time, being had to the *order of time*, and of *discovery*, the better to enable the reader to enter into my views, and trace the actual progress of my thoughts in the several investigations.

For the sake of conciseness, I have not, indeed, troubled the reader with every conjecture and hypothesis which I formerly adopted; but I have not failed to mention the most considerable of them; not being ashamed of the mistakes I have made, and being willing to encourage young adventurers, by shewing them that, notwithstanding many errors (to which even the most sagacious, and the most cautious, are subject) their labours may be crowned with considerable success.

No

No person, I am confident, will now wish that, in order to prevent such mistakes, I had deferred the publication of any of my volumes till I had more nearly completed the courses of experiments, of which they contain an account; and I shall still pursue the same method of *speedy publication*, though the consequence of it should be the necessity, in some future time, of making another new modelled, and better purged edition of all my philosophical writings.

To repeat what I said in the preface of the very first volume of experiments on air; considering the attention which is now given to this subject by philosophers in all parts of Europe, and the rapid progress that has already been made, and may be expected to be made, in this branch of knowledge, all unnecessary delays in the publication of experiments relating to it, are peculiarly unjustifiable.

When, for the sake of a little more reputation, men can keep brooding over a new fact, in the discovery of which they might, possibly, have very little real merit, till they think they can astonish the world with a system as *complete* as it is *new*, and give mankind a high idea of their judgment and penetration; they are justly punished for their ingratitude to the fountain of all knowledge, and for their want

of a genuine love of science and of mankind, in finding their boasted discoveries anticipated, and the field of honest fame pre-occupied, by men, who, from a natural ardour of mind engage in philosophical pursuits, and with an ingenuous simplicity immediately communicate to others whatever occurs to them in their inquiries.

As to myself, I find it absolutely impossible to produce a work on this subject that shall be any thing like *complete*. Every publication I have frankly acknowledged to be very imperfect, and the present, I am as ready to acknowledge, is so. But, paradoxical as it may seem, this will ever be the case in the progress of natural science, so long as the works of God are, like himself, infinite and inexhaustible. In completing one discovery, we never fail to get an imperfect knowledge of others, of which we could have had no idea before; so that we cannot solve one doubt without creating several new ones.

No philosophical investigation can be said to be completed, which leaves any thing unknown that we are prompted by it to wish we could know relating to it. But such is the necessary connection of all things in the system of nature, that every discovery bring to our view many things of which we
had

had no intimation before, the complete discovery of which we cannot help wishing for ; and whenever these discoveries are completed, we may assure ourselves they will farther increase this kind of dissatisfaction.

The greater is the circle of light, the greater is the boundary of the darkness by which it is confined. But, notwithstanding this, the more light we get, the more thankful we ought to be. For by this means we have the greater range for satisfactory contemplation. In time the bounds of light will be still farther extended ; and from the infinity of the divine nature, and the divine works, we may promise ourselves an endless progress in our investigation of them : a prospect truly sublime and glorious. The works of the greatest and most successful philosophers are, on this account, open to our complaints of their being imperfect.

Travelling on this ground resembles Pope's description of travelling among the Alps, with this difference, that here there is not only a *succession*, but an *increase* of new objects and new difficulties.

So pleas'd at first the tow'ring Alps we try,
Mount o'er the vales, and seem to tread the sky.

Th' eternal snöws appear already past,
 And the first clouds and mountains seem the last.
 But those attain'd, we tremble to survey
 The growing labours of the lengthen'd way.
 Th' increasing prospect tires our wand'ring eyes,
 Hills peep o'er hills, and Alps on Alps arise.

ESSAY ON CRITICISM.

Newton, as he had very little knowledge of *air*, so he had few doubts concerning it. Had Dr. Hales, after his various and valuable investigations, given a list of all his *desiderata*, I am confident that he would not have thought of one in ten that had occurred to me at the time of my first publication; and my doubts, queries, and *hints for new experiments*, are very considerably increased, after a series of investigations, which have thrown great light upon many things of which I was not able to give any explanation before.

A person who means to serve the cause of science effectually, must hazard his own reputation so far as to risk even *mistakes* in things of less moment. Among a multiplicity of new objects, and new relations, some will necessarily pass without sufficient attention; but if a man be not mistaken in the principal object of his pursuits, he has no occasion to distress himself about lesser things. In the progress
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of his inquiries he will generally be able to rectify his own mistakes ; or if little and egotistical minds should take a malignant pleasure in detecting them for him, and endeavouring to expose him, he is not worthy of the name of a philosopher, if he has not strength of mind sufficient to enable him not to be disturbed at it. He who does not foolishly affect to be above the failings of humanity, will not be mortified when it is proved that he is but a man.

I do not think it at all degrading to the business of experimental philosophy, to compare it, as I often do, to the diversion of *hunting*, where it sometimes happens that those who have beat the ground the most, and are consequently the best acquainted with it, weary themselves without starting any game ; when it may fall in the way of a mere passenger ; so that there is but little room for boasting in the most successful termination of the chase.

The best founded praise is that which is due to the man, who, from a supreme veneration for the God of nature, takes pleasure in contemplating his *works*, and from a love of his fellow creatures, as the offspring of the same all-wise and benevolent parent ; with a grateful sense and perfect enjoyment of the means of happiness of which he is already possessed, seeks, with earnestness, but without murmuring or

impatience, that greater *command of the powers of nature*, which can only be obtained by a more extensive and more accurate *knowledge* of them ; and which alone can enable us to avail ourselves of the numerous advantages with which we are surrounded, and contribute to make our common situation more secure and happy.

Besides, the man who believes that there is a *governor* as well as a *maker* of the world (and there is certainly equal reason to believe both) will acknowledge his providence and favour at least as much in a successful pursuit of *knowledge*, as of *wealth* ; which is a sentiment that intirely cuts off all boasting with respect to ourselves, and all envy and jealousy with respect to others ; and disposes us mutually to rejoice in every new light that we receive, through whose hands soever it be conveyed to us.

I shall pass for an enthusiast with some, but I am perfectly easy under the imputation, because I am happy in those views which subject me to it ; but considering the amazing improvements in natural knowledge which have been made within the last century, and the many ages, abounding with men who had no other object besides study, in which, however, nothing of this kind was done, there appears to me to be a very particular providence in the concurrence

rence of those circumstances which have produced so great a change ; and I cannot help flattering myself that this will be instrumental in bringing about other changes in the state of the world, of much more consequence to the improvement and happiness of it.

This rapid process of knowledge, which, like the progress of a wave of the sea, of sound, or of light from the sun, extends itself not this way or that way only, but *in all directions*, will, I doubt not, be the means, under God, of extirpating *all* error and prejudice, and of putting an end to all undue and usurped authority in the business of *religion*, as well as of *science* ; and all the efforts of the interested friends of corrupt establishments of all kinds, will be ineffectual for their support in this enlightened age ; though, by retarding their downfall, they may make the final ruin of them more complete and glorious. It was ill policy in Leo X. to patronize polite literature. He was cherishing an enemy in disguise. And the English hierarchy (if there be any thing unsound in its constitution) has equal reason to tremble even at an air pump, or an electrical machine.

This is not now a business of *air* only, as it was at the first ; but appears to be of much greater mag-

nitude and extent, so as to diffuse light upon the most *general principles* of natural knowledge, and especially those about which *chemistry* is particularly conversant. And it will not now be thought very assuming to say, that, by working in a tub of water, or a basin of quicksilver, we may perhaps discover principles of more extensive influence than even that of *gravity* itself, the discovery of which, in its full extent, contributed so much to immortalize the name of Newton.

I would, however, caution my reader not to be too sanguine in his expectations from the happy train which this branch of philosophy seems to be in. Considering the unexampled rapidity with which discoveries have hitherto been made in it, the number of persons in many and distant countries now engaged in these pursuits, and the emulation that is necessarily excited in such circumstances; and considering, at the same time, how nearly this subject is allied to the most general and comprehensive laws of nature with which we are acquainted; some may be apt to imagine, that every year must produce discoveries equal to all that were made by a Newton or a Boyle; and I am far from saying that this may not be the case, or that it is very improbable.

But;

But, though I have little doubt, from the train that things are visibly in, that philosophical discoveries in general will go on with an accelerated progress (as indeed they have done ever since the revival of letters in Europe) it would be too rash to infer, from the present flattering appearances, that any particular expedition into the undiscovered regions of science will be crowned with more distinguished success than another. Nothing is more common, in the history of all the branches of experimental philosophy, than the most unexpected revolutions of good or bad success. In general, indeed, when numbers of ingenious men apply themselves to one subject, that has been *well opened*, the investigation proceeds happily and equably. But, as in the history of *electricity*, and now in the discoveries relating to *air*, light has burst out from the most unexpected quarters, in consequence of which the greatest masters of science have been obliged to recommence their studies, from new and simpler elements; so it is also not uncommon for a branch of science to receive a check, even in the most rapid and promising state of its growth.

It is true that the rich and the great in this country give less attention to these subjects than, I believe, they were ever known to do, since the time of Lord Bacon, and much less than men of rank
and

and fortune in other countries give to them. But with us this loss is made up by men of leisure, spirit, and ingenuity, in the middle ranks of life, which is a circumstance that promises better for the continuance of this progress in useful knowledge than any noble or royal patronage. With us, also, politics chiefly engage the attention of those who stand foremost in the community, which, indeed, arises from the *freedom* and peculiar *excellence* of our constitution, without which even the spirit of men of letters in general, and of philosophers in particular, who never directly interfere in matters of government, would languish.

It is rather to be regretted, however, that, in such a number of nobility and gentry, so very few should have any taste for scientific pursuits, because, for many valuable purposes of science, *wealth* gives a decisive advantage. If extensive and lasting *fame* be at all an object, literary, and especially scientific pursuits, are preferable to political ones in a variety of respects. The former are as much more favourable to the display of the human faculties than the latter, as the *system of nature* is superior to any *political system* upon earth.

If extensive *usefulness* be the object, science has the same advantage over politics. The greatest
success

success in the latter seldom extends farther than one particular country, and one particular age; whereas a successful pursuit of science makes a man the benefactor of all mankind, and of every age. How trifling is the fame of any statesman that this country has ever produced to that of Lord Bacon, of Newton, or of Boyle; and how much greater are our obligations to such men as these, than to any other in the whole *Biographia Britannica*; and every country, in which science has flourished, can furnish instances for similar observations.

Here my reader will thank me, and the writer will, I hope, forgive me, if I quote a passage from the postscript of a letter which I formerly received from that excellent, and in my opinion, not too enthusiastical philosopher, father Beccaria, of Turin.

Mi spiace che il mondo politico, eh' è pur tanto passeggero, rubbi il grande Franklin al mondo della natura, che non sa ne cambiare, ne mancare. In English.

“ I am sorry that the *political world*, which is so
 “ very transitory, should take the great Franklin
 “ from the *world of nature*, which can never change,
 “ or fail.”

Scientific pursuits have such an advantage over most others, as ought more especially to recommend them

them to persons of rank and fortune. They never fail to furnish materials for the most agreeable and active pursuits, and such as are, at the same time, in the highest degree, useful and honourable, and are, by this means, capable of doing unspeakably more for them than the largest fortunes can do without this resource. Were persons thus engaged, there would be less temptation to have recourse to pleasure and dissipation, for the employment of their vacant time; and such pursuits would be particularly valuable to those who have no *talent* for politics, or any proper *call*, to occupy themselves in public affairs. Besides, the last is a path in which, from the nature of things, only a very few can walk; and the former, viz. a course of vicious pleasure, it is much to be lamented that any human being should tread.

Man is a being endued by his creator with excellent faculties, and not to have *serious objects of pursuit* is to debase and degrade himself. It is to rank himself with beings of a lower order, aiming at nothing that is much higher than the low pleasures they are capable of; at the same time that, from the remains of nobler powers, of which he cannot wholly divest himself, he is incapable of that unallayed enjoyment of sensual pleasures that brutes have.

I am

I am sorry to have occasion to observe, that natural science is very little, if at all, the object of *education* in this country, in which many individuals have distinguished themselves so much by their application to it. And I would observe that, if we wish to lay a good foundation for a philosophical taste, and philosophical pursuits, persons should be accustomed to the sight of experiments, and processes, in *early life*. They should, more especially, be early initiated in the theory and practice of *investigation*, by which many of the old discoveries may be made to be really *their own*; on which account they will be much more valued by them. And, in a great variety of articles, very young persons may be made so far acquainted with every thing necessary to be previously known, as to engage (which they will do with peculiar alacrity) in pursuits truly original.

At all events, however, the curiosity and surprize of young persons should be excited as soon as possible; nor should it be much regarded whether they properly understand what they see, or not. It is enough, at the first, if striking facts make an impression on the mind, and be remembered. We are, at all ages, but too much in haste to *understand*, as we think, the appearances that present themselves to us. If we could content ourselves with the bare
I knowledge

knowledge of new *facts*, and suspend our judgment with respect to their *causes*, till, by their analogy, we were led to the discovery of more facts, of a similar nature, we should be in a much surer way to the attainment of real knowledge.

I do not pretend to be perfectly innocent in this respect myself; but I think I have as little to reproach myself with on this head as most of my brethren; and whenever I have drawn general conclusions too soon, I have been very ready to abandon them, as all my publications, and this work in particular, will evidence. I have also repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to consider new *facts* only as *discoveries*, and mere *deductions* from those facts, as of no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves.

I also cannot help expressing a wish that during the establishment of peace in Europe (and happily it is not in the power of any state to be always at war) we may see every obstruction to the progress of knowledge, which is equally friendly to all states, removed. Taxes on the importation of books, and other articles of literature, are so impolitic, as well as illiberal, that it is earnestly wished that something
may

may be stipulated by contending powers for abolishing them. There are statesmen whose minds are sufficiently enlarged to see that philosophy gives an ample equivalent for the exemption.

I might enlarge much more than I have done in this preface on the *dignity*, and *utility*, of experimental philosophy; but shall only observe farther, that it is nothing but a superior knowledge of the laws of nature, that gives Europeans the advantage they have over the Hottentots, or the lowest of our species. Had these people never known Europeans, they could not have formed an idea of any mode of life superior to their own, though it differs but little from that of the brutes. In like manner, science advancing, as it does, with an accelerated progress, it may be taken for granted, that mankind some centuries hence will be as much superior to us in knowledge, and improvements in the arts of life, as we now are to the Hottentots, though we cannot have any conception what that knowledge, or what those improvements, will be. It is enough for us to see that nature is inexhaustible, that it is a rich mine, in which we shall never dig in vain, and that it is open to infinitely more labourers than are now employed in exploring its contents, or in digging for them.

Having

Having been a pretty successful adventurer in this great mine, my philosophical friends in general wonder that I do not confine my attention to it. Their dissatisfaction with me is so great, and I hear of it from so many quarters, that I think it right to take some opportunity (and a better than the present will hardly occur) to make an apology for my conduct, especially to those of my friends by whose assistance I am enabled to give my time to these liberal pursuits; being pleased to think that my attention to them will be of some advantage to science and the public.

In the first place, I would observe, that I follow my own best judgment in devoting my time to what I really apprehend to be the most important pursuits, those from which myself, and mankind at large, will finally derive the greatest advantage; and I must be allowed to say, that the greater variety of objects to which it is evident that I have given attention, must qualify me to be a better judge in this case than those who censure my conduct. Persons who have only one object of pursuit, never fail to over-rate it, and of course to undervalue other things. I would farther observe, that the attention I have given to *theology* (which, by the way, is my original and proper province, and for which I may, therefore, be allowed to have a justifiable

justifiable predilection) does not engross so much of my time as some persons may imagine. I am particularly complained of at present, as having thrown away so much time on the composition of my *History of the Corruptions of Christianity*, of the *Opinions concerning Christ*, and of the *Christian Church in general*. But I can assure them, and the nature of the thing, if they consider it, may satisfy them, that the time I must necessarily have bestowed upon the experiments of which an account is contained in any one of six volumes, is much more than I have given to three or four of those of which the other consist, and to all the controversial pieces that I have written in defence of them. In general, during the composition of those works, the greatest part of every day was spent in my laboratory, and the evenings and mornings only in reading or writing. Besides, these different studies so relieve one another, that I believe I do more in each of them, by applying to them alternately, than I should do, if I gave my whole attention to one of them only.

But my principal defence rests on the superior dignity and importance of *theological studies* to any other whatever, and with some observations of this kind I shall chuse to conclude this long Preface.

Every rational being ought to distinguish, by the greater attention that he gives to them, those objects which are of the greatest importance to himself, and to mankind at large. And certainly, if there be any just rule for estimating the value of a problem, or query, that is proposed to us, we must think it of infinitely more moment to discover whether there be a future, and especially an endless, life after this, and how to secure a happy lot in that future life, than to make the best provision possible for ourselves in this life, which is the ultimate object of all natural philosophy. Studies, but remotely connected with that great object, must have a dignity and importance infinitely superior to any other. A man must never have thought a moment on the subject, if he hesitate to give a decided preference in the case. To think or act otherwise, would be like a man busying himself about farthings, who has large estates, or kingdoms, depending, and who should neglect the latter in order to secure the former.

All that any philosophical person can pretend to say in the case, must be, that the expectation of a future life is so manifestly chimerical, that it can never be worth a wise man's while to lose a moment in thinking about it, or to employ his time in any study relating to it. This I know to be the opinion

nion of many who will read this book, if not this preface. But in this I must take the liberty to differ from them, and for reasons which I shall submit to their serious consideration.

Natural phenomena, I agree with them, are unfavourable to any expectation of a future life, and the doctrine of an *immaterial soul*, capable of subsisting and acting when the body is in the grave (on which the doctrine of a future state is generally founded) I am as fully persuaded as they can be, is unauthorized by any natural appearances whatever. My expectation of a future life rests on another foundation; and, improbable as I acknowledge the doctrine to be, according to the light of nature, it is nevertheless such as I firmly believe, on the plainest of all evidence; the author of nature having given us an absolute assurance of it, by persons authorized to speak in his name, and whose divine mission was proved by such works as no other than the author of nature could have enabled them to perform.

That such works have been performed, and for this important purpose, must, I apprehend, be true, if there be any truth in history. And there is no kind of evidence more easily subjected to a rigorous examination than that which is of the historical kind,

the maxims of which we are every day conversant with.

Now it appears to me, that we must either admit the truth of the gospel history, which contains an account of the doctrine, miracles, death, and resurrection of Christ (on which the belief of a future life depends) or believe what is infinitely more incredible, viz. that several thousand people, present at the transactions, and who had no motive to believe them without sufficient evidence, but every motive to turn their eyes from them, or disbelieve them if they could, should yet, without such evidence, have given the firmest assent to them, and have entertained so little doubt of the extraordinary facts, as to maintain their faith in them at the hazard of every thing dear to them in life, and even cheerfully lay down their lives, rather than abandon their faith. Let philosophers, as such, account for this great *fact*, without admitting more real miracles, and those of a more extraordinary kind, than the belief of christianity requires of me, and I will relinquish my present faith, dear as it is to me, and join them in exposing it.

As philosophers, the question between us is, whose faith, strictly speaking, is more agreeable to *present appearances*. Whatever we may think of an *author*
of

of nature, and of his attention to it, we equally believe in *the uniformity of the laws of nature*, and that *man*, whose constitution is a part of the system of nature, was the same kind of being two thousand years ago that he is now; as much as that a horse of that age, or an oak tree of that age, had the same properties with the horses and oaks of the present. Consequently, whatever was possible with respect to *man* in any former period, is equally possible now.

But will any man, who gives a moment's attention to the subject, say that it is even *possible* that several thousand persons, in London or Paris, could be made to believe that any man in London or Paris, died and rose from the dead in their own life-time, that they should persist in this persuasion through life, without shewing any sign of insanity, that they should gain numerous profelytes to their opinion, though it subjected all who embraced it to all kinds of persecution, and even to death; and that the belief of it should establish itself against all opposition, without any person being able to detect the imposition?

Now I apprehend that this might take place more easily in London, or in Paris, at this day, than it could have done at Jerusalem in the time

of our Saviour. Human nature could not have been the same thing then that we find it to be at present, if mankind could have been so imposed upon. This I therefore think absolutely incredible, and consequently, as the less difficulty of the two, as believing a thing much less improbable, I admit the truth of the gospel history, the admission of which makes the subsequent account of the propagation of christianity (which all history, and even the present state of things, proves to be true) perfectly easy and natural. Admitting these leading facts, all the rest follows of course, and all things came to be as they are without any farther miracle. But real miracles we must have somewhere, in order to account for the present state of things; and if we must admit miracles, let them be such as have a *great object*, and not such as have no object at all, but only serve to puzzle and confound us.

The history of the Jews, and the books of the Old Testament, furnish many *facts*, which no hypothesis besides that of the divine origin of their religion can explain. Let the philosopher only admit as a *postulatum* that Jews are, and always were, *men*, constituted as other men are, and let him not deceive himself, by considering them as beings of another species. All I wish in this respect

is, that persons who pretend to the character of *philosophers*, would be so throughout, and carry the same spirit into the study of history, and of human nature, that they do into their laboratories; first assuring themselves, with respect to *facts*, and then explaining those facts by reducing them to *general principles* (which, from the uniformity of nature, must be universally true), and then I shall have no doubt of their becoming as firm believers in christianity as myself. They will find no other *hypothesis*, that can explain such appearances as they cannot deny to be real. Let philosophers now say, whether there be reason in this, or not.

I therefore take the liberty, having been led to advance thus much, to address my brother philosophers on a subject equally interesting to us as *philosophers*, and as *men*. Do not disregard a question of infinite moment. Give it that degree of attention to which it is naturally intitled; and especially do not so far abandon the serious character of *philosophers*, as to *laugh* where you ought to *reason*. At least, do this great subject, and yourselves, the justice to consider the *facts*, and endeavour to frame some *hypothesis* by which to account for them; and do not decide in half an hour, on an inquiry which well deserves the study of a great part of your lives.

If I have a stronger bias than many other persons in favour of christianity, it is that which philosophy gives me. I view with rapture the glorious face of nature, and I admire its wonderful constitution, the laws of which are daily unfolding themselves to our view. It is but little that the life of man permits us to see at present, and therefore I feel a most eager desire to renew my acquaintance with it hereafter, and to resume those inquiries with which I am so much delighted now, and which must be interrupted by death.

Could I imagine that the knowledge of nature would ever be exhausted, and that we were approaching to a termination of our enquiries, I could more contentedly shut my eyes on a scene in which nothing more was to be seen, or done. But to quit the stage at present (and I believe the aspect of things will be exactly similar in any future period of our existence) without the hope of re-visiting it, would fill me with the deepest regret. The general who, like Epaminondas, or Wolfe, dies in the arms of victory, dies with satisfaction; but not so he that is cut off in the beginning of a doubtful, though promising, engagement. Thus I feel on the idea of ceasing to breathe, when I have but just begun to know what it is that I breathe.

M. Herschell's late discoveries in, and beyond, the bounds of the solar system, the great views that he has given us of the arrangement of the stars, their revolutions, and those of the immense systems into which they are formed, are peculiarly calculated to inspire an ardent desire of seeing so great a scene a little more unfolded. Such discoveries as these, give us a higher idea of the value of our being, by raising our ideas of the *system* of which we are apart, and, with this, an earnest wish for the continuance of it.

Besides, *civil society* is but in its infancy, the world itself is but very imperfectly known to the civilized inhabitants of it, and we are but little acquainted with the real value of those few of its productions of which we have some knowledge, and which we are only beginning to name, and to arrange. How must a *citizen of the world* wish to know the future progress of it?

To have no wish of this kind certainly argues a low, an ignoble, and I will say, an unphilosophical mind. I consider all such persons, how superior soever they may be to myself in other respects, with pity and concern. They would have unspeakably more satisfaction in their philosophical pursuits, if they carried them on with the views of things that
I have

I have. It has been justly observed, that great views indicate, and indeed constitute, great minds. What elevation of mind, then, would the prospects of the christian, add to those of the philosopher!*

With men of reflection this apology for my conduct will, I doubt not, be admitted as satisfactory; and till I hear better reasons than have yet been offered to me for changing my conduct, I shall continue to give my attention to my different pursuits, according to my own ideas of their respective importance; and my friends have no reason to fear that I shall neglect *philosophy*. It has, perhaps, but too strong charms for me. I shall endeavour, however, to keep it in its proper place, and not so

* If any of my philosophical friends should be induced, by what I have here urged, to look into my *theological writings*, I would take the liberty to recommend to them my *Letters to a Philosophical Unbeliever*, the *Institutes of natural and revealed Religion*, the *General History of the Christian Church, till the Fall of the Western Empire*, and the *History of the Corruptions of Christianity*, especially the *Conclusion*, Part I. relating to Mr. GIBBON, who has declined engaging in the discussion I there proposed to him. If they wish to see more particularly in what manner christianity came to be encumbered with the doctrine of *the trinity*, which has been the foundation of one of the greatest objections to it, I would further refer them to my *History of early Opinions concerning Christ*, where they will see it traced to its proper source in the Platonic philosophy, and where it is proved that the primitive christian church was unquestionably unitarian.

much

much attach myself to the study of the laws which govern *this* world, as to lose sight of the subserviency of this world, and of all things in it, to *another* and a better ; in which I hope to resume these pleasing philosophical pursuits, and to see, in a comprehensive view, those detached discoveries which we are now making here.

At present all our *systems* are in a remarkable manner unhinged by the discovery of a multiplicity of *facts*, to which it appears difficult, or impossible, to adjust them. We need not, however, give ourselves much concern on this account. For when a sufficient number of new facts shall be discovered (towards which even imperfect hypotheses will contribute) a more *general theory* will soon present itself ; and perhaps to the most incurious and least sagacious eye. Thus, when able navigators have, with great labour and judgment, steered towards an undiscovered country, a common sailor, placed at the mast head, may happen to get the first sight of the land. Let us not, however, contend about *merit*, but let us all be intent on forwarding the *common enterprize*, and equally enjoy any progress we may make towards succeeding in it ; and above all, let us acknowledge the guidance of that Great Being, *who has put a spirit in man, and whose inspiration giveth him understanding.*

I have

I have not, in this edition, given a *summary view of facts*, such as I gave in the *fifth* of the preceding volumes, partly because I found it would have made the last volume of a disproportionate size, but chiefly because the arrangement of the present work, and the *Index* to the whole, rendered it less necessary. Such a summary will be found in Mr. Keir's Chemical Dictionary, and in Elementary Treatises, comprizing what all experimenters on air have discovered.

• As I wish to preserve the memory of my patrons (though I hope to do in a more effectual manner than this) I would observe that the six original volumes were inscribed to the following persons; viz. the Marquis of Lansdown, Sir George Savile, the late Earl of Stanhope, Sir John Pringle, Doctor Heberden, and William Constable, Esq. of Burton Constable.

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T H E

INTRODUCTION.

SECTION I.

*A general view of PRECEDING DISCOVERIES relating
to air.*

FOR the better understanding of the experiments and observations on different kinds of air contained in this treatise, it will be useful to those who are not acquainted with the history of this branch of natural philosophy, to be informed of those facts which had been discovered by others, before I turned my thoughts to the subject; which suggested, and by the help of which I was enabled to pursue, my enquiries. Let it be observed, however, that I do not profess to recite in this place *all* that had been discovered concerning air, but only those discoveries the knowledge of which is necessary, in order to understand what I have done myself; so that any person who is only acquainted with the general principles of natural philosophy, may

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be able to read this treatise, and, with proper attention, to understand every part of it.

That the air which constitutes the atmosphere in which we live has *weight*, and that it is *elastic*, or consists of a compressible and dilatable fluid, were some of the earliest discoveries that were made after the dawning of philosophy in this western part of the world.

Also Van Helmont, and other chymists who succeeded him, were acquainted with the property of some *vapours* to suffocate, and extinguish flame, and of others to be ignited; effects, indeed, which could not but have been known in all ages. But they had no idea that the substances (if, indeed, they knew that they were *substances*, and not merely *properties*, and *affections* of bodies which produced those effects) were capable of being separately exhibited in the form of a *permanently elastic vapour*, not condensable by cold, to which I give the name of *air*, any more than the thing that constitutes *smell*. In fact, they knew nothing at all of any air besides *common air*, and therefore they applied the term to no other substance whatever.

That elastic fluids, differing essentially from the air of the atmosphere, but agreeing with it in the properties of weight, elasticity, and transparency, might be generated from solid substances, was discovered by Mr. Boyle, through two remarkable
4 kinds.

kinds of factitious air, at least the effects of them, had been known long before to all miners. One of these is heavier than common air. It lies at the bottom of pits, extinguishes candles, and kills animals that breathe it, on which account it had obtained the name of the *choke damp*. The other is lighter than common air, taking its place near the roofs of subterraneous places; and because it is liable to take fire, and explode, like gunpowder, it had been called the *fire damp*. The word *damp* signifies *vapour* or *exhalation* in the German and Saxon language.

Mr. Boyle was, I believe, the first who discovered that what we now call *fixed air*, and also *inflammable air*, are really *elastic fluids*, capable of being exhibited in a state unmixed with common air, a fact which nothing that was known before his time could have given him the least reason to expect; nor, in fact, did he make the discovery by any kind of reasoning *a priori*. It was the unexpected result of his experiments.

Though the former of these kinds of air had been known to be noxious, the latter, I believe, had not been discovered to be so; having always been found, in its natural state, so much diluted with common air, as to be breathed with safety. Air of the former kind, besides having been discovered in various caverns, particularly the *grotta del Cane* in Italy,

had also been observed on the surface of fermenting liquors, and had been called *gas* (which is the same with *geist*, or *spirit*) by Van Helmont, and other German chymists ; but afterwards it obtained the name of *fixed air*, especially after it had been discovered by Dr. Black of Edinburgh to exist, in a fixed state, in alkaline salts, chalk, and other calcareous substances.

This excellent philosopher discovered that it is the presence of the fixed air in these substances that renders them *mild*, and that when they are deprived of it, by the force of fire, or any other process, they are in that state which had been called *caustic*, from their corroding or burning animal and vegetable substances.

Fixed air had been discovered by Dr. Macbride of Dublin, after an observation of Sir John Pringle's, which led to it, to be in a considerable degree antiseptic ; and since it is extracted in great plenty from fermenting vegetables, he had recommended the use of *wort* (that is an infusion of malt in water) as what would probably give relief in the sea-scurvy, which is said to be a putrid disease.

Dr. Brownrigg had also discovered that the same species of air is contained in great quantities in the water of the Pyrmont spring at Spa in Germany, and in other mineral waters, which have what is called an *acidulous* taste, and that their peculiar
flavour,

flavour, briskness, and medicinal virtues, are derived from this ingredient.

Dr. Hales, without seeming to imagine that there was any material difference between these kinds of air and common air, observed that certain substances and operations *generate* air, and others *absorb* it; imagining that the diminution of air was simply a taking away from the common mass, without any alteration in the properties of what remained. His experiments, however, are so numerous, and various, that they are justly esteemed to be the solid foundation of all our knowledge of this subject.

Mr. Cavendish had exactly ascertained the specific gravities of fixed and inflammable air, shewing the former of them to be $1\frac{1}{2}$ heavier than common air, and the latter ten times lighter. He also shewed that water would imbibe more than its own bulk of fixed air.

Lastly, Mr. Lane discovered that water thus impregnated with fixed air will dissolve a considerable quantity of iron, and thereby become a strong chalybeate.

Besides these two kinds of factitious air, that which I call *nitrous air* obtruded itself upon Dr. Hales; but even he, as I observed, had no idea of there being more than *one kind of air*, loaded with different vapours; and was far from imagining that they differed from one another so very essentially as

they are now known to do. And though Mr. Boyle, Dr. Hales, and others, could not but be acquainted with the effluvium of *spirit* of salt, and also of *volatile alkali*, they could have no idea that the substance which had those powers was capable of being separated from common air, and of being exhibited free from moisture, in the form of a permanently elastic vapour, to appearance exactly like that which constitutes the common atmosphere. Or if any person, till within these very few years, had such a notion (of which, however, I do not believe that they have given the least intimation) it must have been a mere *random conjecture*, and what nothing but actual experiment could have ascertained.

Even Mr. Cavendish, whose experiments relating to air immediately preceded my own, appears not to have had so much as a suspicion of this kind. For he relates an experiment of his, on the solution of copper in the marine acid, as inexplicable, except on the hypothesis of there being a kind of *air that lost its elasticity by the contact of water*, which admits of the easiest solution imaginable, on the supposition of the spirit of salt emitting a vapour, which though capable of being confined by quicksilver, and of being by that means exhibited in the form of air, was instantly *absorbed* by water, which would thereupon become possessed of all the properties of common spirit of salt,

In

In fact, none of the chymists appear to have had the least idea of its being even possible to separate the acid or alkaline principles from the water with which they are always found combined; and therefore, though they did suppose them capable of farther *concentration*, they still considered a certain portion of water, as absolutely *essential* to them; and consequently all the experiments that have hitherto been made on the affinities of the acids, and alkalis, are, in fact, nothing more than the affinities of *compound substances*, consisting of the *acids* or *alkali*, and *water*.

The above-mentioned, I would observe, are by no means all the discoveries concerning air that have been made by the gentlemen whose names I have mentioned, and still less are they all that have been made by others; but they comprize all the previous knowledge of this subject that is necessary to the understanding of this treatise; except a few particulars, which will be mentioned in the course of the work, and which it is, therefore, unnecessary to recite in this place.

SECTION II.

Of the Use of Terms.

IN writing on the subject of *different kinds of air*, I found myself at a loss for proper *terms*, by which to distinguish them, those which have hitherto obtained being by no means sufficiently characteristic, or distinct. The only terms in common use were, *fixed air*, *mephitic*, and *inflammable*. The last, indeed, sufficiently characterizes and distinguishes that kind of air which takes fire, and explodes on the approach of flame; but it might have been termed *fixed* with as much propriety as that to which Dr. Black, and others before him, had given that denomination; since it is originally part of some solid substance, and exists in an unelastic state.

The term *mephitic* is equally applicable to what is called *fixed air*, to that which is *inflammable*, and to many other kinds; since they are equally noxious, when breathed by animals. Rather, however, than either introduce new terms, or change the signification of old ones, I have used the term *fixed air*, in the sense in which it is now commonly used, and have distinguished the other kinds by their properties, or some other periphrasis. I have been under

a necessity, however, of giving names to those kinds of air, to which no names had been given by others, as *nitrous*, *acid*, *alkaline*, &c.

No person was ever more temperate, or more cautious, than I have been in the introduction of *new terms*, considering the number of *new facts* that I have discovered. It was with great hesitation, though compelled by necessity, that I did it at all; generally with the advice of my most judicious friends, and always adopting such as were analogous to others in established use. Thus when I found the terms *common or atmospherical air*, *fixed air*, and *inflammable air*, used by all philosophers, and no person whatever had objected to them, it was certainly natural for me to continue to apply the term *air* to other *elastic transparent fluids*, not condensable by cold, and to distinguish them by other appellations, drawn from the peculiar circumstances of their production, as *nitrous air*, *acid air*, *alkaline air*, *phlogisticated* and *dephlogisticated air*, &c. using the term *air* as expressive of the mere *form* in which a substance is exhibited, without any consideration of the elements of which it consists. I therefore think the term *gas*, which many use, in this sense, to be unnecessary; the term *air*, as it had long been used by philosophers, being sufficient for the purpose.

They

They who chuse to apply the term *air* to a *substance*, and not to a *form*, are certainly at full liberty so to do, if they please; and provided we understand one another, no inconvenience will result from our use of a different language. But then the same persons should be uniform in their objections and practice, and call nothing by the name of *air* that they do not believe to consist of that one *elementary substance* to which they profess to appropriate the term. The language that I adopt, in this respect, implies no attachment to any hypothesis whatever, and may still be used though I should change my opinion on that subject; which is certainly a very great advantage in philosophical language. In adopting the terms *phlogisticated* and *dephlogisticated* air, I did not, I own, use the same judgment; but as by good fortune, they do not appear at all improper, I do not see any sufficient reason to abandon them. The *azote* in the new nomenclature is not expressive of any thing peculiar to what I have called phlogisticated air; and the term *vital*, does not sufficiently distinguish dephlogisticated from *common*, or *atmospherical* air.

Some persons more particularly object to the term *air*, as applied to *acid*, *alkaline*, and even *nitrous air*; but it is certainly very convenient to have a common term by which to denote things which
have

have so many common properties, and those so very striking ; all of them agreeing with the air in which we breathe, and with *fixed air*, in *elasticity*, and *transparency*, and in being alike affected by heat or cold ; so that to the eye they appear to have no difference at all. With much more reason, as it appears to me, might a person object to the common term *metal*, as applied to things so very different from one another as gold, quicksilver, and lead.

Besides, acid and alkaline air do not differ from common air (in any respect that can countenance an objection to their having a common appellation) except in such properties as are common to it with fixed air, though in a different degree ; viz. that of being imbibed by water. But, indeed, all kinds of air, common air itself not excepted, are capable of being imbibed by water in some degree.

Some may think the terms acid and alkaline *vapour* more proper than acid and alkaline *air*. But the term *vapour* having always been applied to elastic matters capable of being condensed in the temperature of the atmosphere, especially the vapour of water, it seems harsh to apply it to any elastic substance, which at the same time that it is as transparent as the air we breathe, is no more affected by cold than it is.

SECTION III.

An account of the APPARATUS with which the following experiments were made.

RATHER than describe at large the manner in which every particular experiment that I shall have occasion to recite was made, which would both be very tedious, and require an unnecessary multiplicity of drawings, I think it more adviseable to give, at one view, an account of all my apparatus and instruments, or at least of every thing that can require a description, and of all the different operations and processes in which I employ them.

It will be seen that my apparatus for experiments on air is, in fact, nothing more than that of Dr. Hales, Dr. Brownrigg, and Mr. Cavendish, diversified, and made a little more simple.

For experiments in which air will bear to be confined by water, I first used an oblong trough made of earthen ware, as *a* Plate I. fig. 1. about eight inches deep, at one end of which I put thin flat stones, about an inch, or half an inch, under the water, using more or fewer of them according to the quantity of water in the trough. I afterwards found it more convenient to use a larger wooden trough,
of

of the same form, with a shelf about an inch lower than the top, instead of the flat stones above-mentioned. But I now use a trough, two feet two inches long, one foot two inches wide, and nine inches deep, for common purposes, and others of different dimensions for particular uses. In making them the joints are fixed in fresh paint, which renders them perfectly water tight.

In one end of this trough are *ledges*, on which it can slide, so that I can take it out with pleasure; I have also a *shelf* like Fig. 1. Plate III. except that it is not suspended, as that is, by thin pieces of copper, bended into the form of hooks, which, however, answered very well. The shelf is about an inch and an half in thickness, for the convenience of excavating the under-side in the form of *funnels*, the orifices of which, about a quarter of an inch in diameter, appear on the upper side, as the form and size of the cavity below is expressed by the dots above. This was an ingenious contrivance of the Duc de Chaulnes.

These funnels should be made as capacious as possible; but care should more especially be taken, that no part of them be too flat, lest any bubbles of air should be retained, and not pass into the vessels placed to receive them.

When fresh air is generated, it is convenient to introduce the tube of the phial in which it is produced, quite under the shelf, into the hollow of
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the funnel. But when it happens that the sweep of the tube is too short for that purpose, I make use of a small production of the upper part of the shelf, with a slit in it, under which the shorter tube may be brought; and the edge of the jar that receives the air, may be made to slide over the place at which the bubbles issue.

Fig. 2. Plate III. is a side view of a glass funnel supported by a wooden pillar, rising from a base, to which a plate of lead is fastened, in order to make it sink, and keep its place in the water. At the top of the pillar is a piece of wood cut in front (but, for that reason, not visible in this figure) in a concave form, for supporting a glass tube, that, resting on the orifice of the funnel, may lean against it. Both this piece of wood, and also that which supports the funnel, are made to slide up and down, and are fixed by wedges at whatever height is found to be most convenient. This apparatus saves the trouble and inconvenience of keeping one's hand in the water for the sake of holding the funnel, while the air is pouring through it.

Fig. 3. Pl. III. represents an apparatus that would not deserve a copper-plate, but that there is often great convenience in little things. It exhibits a basin of water, or quicksilver, so placed, in a frame of wood, as to contain several glass tubes, which may be supported with little trouble, and disposed of without materially interfering with each other. In
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this manner I have often more than half a dozen in use at the same time.

After using this *bason* for quicksilver, which, on many accounts, is, in general, more convenient than any other form of a *reservoir*. I found I had had occasion to transfer air from one jar to another in quicksilver, in the same manner as I had used to do in water; and then I found it absolutely necessary for this purpose, to make use of an oblong *trough*, Pl. V. fig. 1. That which I have commonly used is made of wood, seven inches long, three wide, and three deep, made cylindrical at the bottom, in order to make the least quantity of of quicksilver necessary. But I have an upright piece of wood at one end, contrived to support tall glass vessels without danger of falling. It is only with such an apparatus as this, that given quantities of alkaline and acid airs can be mixed, as is described in the course of the work.

The several kinds of air I usually keep in *cylindrical jars*, as *c, c*, Pl. I. fig. 1, about ten inches long, and two and an half wide, being such as I have generally used for electrical batteries; but I have likewise vessels of very different forms and sizes, adapted to particular experiments.

When I want to remove vessels of air from the large trough, I place them in *pots* or *dishes*, of various sizes, to hold more or less water, according to the time that I have occasion to keep the air, as
fig.

fig. 2. These I plunge in water, and slide the jars into them; after which they may be taken out together, and be set wherever it shall be most convenient. For the purpose of merely removing a jar of air from one place to another, where it is not to stand longer than a few days, I make use of common *tea-dishes*, which will hold water enough for that time, unless the air be in a state of diminution, by means of any process that is going on in it.

If I want to try whether an animal will live in any kind of air, I first put the air into a small vessel, just large enough to give it room to stretch itself; and as I generally make use of mice for this purpose, I have found it very convenient to use the hollow part of a tall beer-glass, *d* Fig. 1, which contains between two and three ounce measures of air. In this vessel a mouse will live twenty minutes, or half an hour.

For the purpose of these experiments it is most convenient to catch the mice in small wire traps, out of which it is easy to take them, and, holding them by the back of the neck, to pass them through the water into the vessel which contains the air. If I expect that the mouse will live a considerable time, I take care to put into the vessel something on which it may conveniently sit, out of the reach of the water. If the air be good, the mouse will soon be perfectly at its ease, having suffered nothing by its passing through the water. If the

air be supposed to be noxious, it will be proper (if the operator be desirous of preserving the mice for farther use) to keep hold of their tails, that they may be withdrawn as soon as they begin to shew signs of uneasiness; but if the air be thoroughly noxious, and the mouse happens to get a full inspiration, it will be impossible to do this before it be absolutely irrecoverable.

In order to *keep* the mice, I put them into receivers open at the top and bottom, standing upon plates of tin perforated with many holes, and covered with other plates of the same kind, held down by sufficient weights, as Pl. I. fig. 3. These receivers stand upon a *frame of wood*, that the fresh air may have an opportunity of getting to the bottoms of them, and circulating through them. In the inside I put a quantity of paper or tow, which must be changed, and the vessel washed and dried, every two or three days. This is most conveniently done by having another receiver, ready cleaned and prepared, into which the mice may be transferred till the other shall be cleaned.

Mice must be kept in a pretty exact temperature, for either much heat or much cold kills them presently. The place in which I have generally kept them, was a shelf over the kitchen fire-place, where, as it is usual in Yorkshire, the fire never goes out; so that the heat varies very little, and I find it to be,

at a medium, about 70 degrees of Fahrenheit's thermometer. When they had been made to pass through the water, as they necessarily must be in order to a change of air, they require, and will bear, a very considerable degree of heat, to warm and dry them.

N.B. I found, to my great surprize, in the course of these experiments, that mice will live intirely without water ; for though I have kept them for three or four months, and have offered them water several times, they would never taste it ; and yet they continued in perfect health and vigour. Two or three of them will live very peaceably together in the same vessel ; though I had one instance of a mouse tearing another almost in pieces, and when there was plenty of provisions for both of them.

In the same manner in which a mouse is put into a vessel of any kind of air, a *plant*, or any thing else, may be put into it, viz. by passing it through the water ; and if the plant be of a kind that will grow in water only, there will be no occasion to set it in a pot of earth, which will otherwise be necessary.

There may appear, at first sight, some difficulty in opening the mouth of a phial, containing any substance, solid or liquid, to which water must not be admitted, in a jar of any kind of air, which is an operation that I have sometimes had recourse to ; but this I easily effect by means of a *cork cut tapering,*
and

and a strong wire thrust through it, as in fig. 4, for in this form it will sufficiently fit the mouth of any phial, and by holding the phial in one hand, and the wire in the other, and plunging both my hands in the trough of water, I can easily convey the phial through the water into the jar, which must either be held by an assistant, or be fastened by strings, with its mouth projecting over the shelf. When the phial is thus conveyed into the jar, the cork may easily be removed, and may also be put into it again at pleasure, and conveyed the same way out again.

When any thing, as a gallipot, &c. is to be supported at a considerable height within a jar, it is convenient to have such *wire stands* as are represented fig. 5. They answer better than any other, because they take up but little room, and may be easily bended to any shape or height.

If I have occasion to pour air from a vessel with a wide mouth into another with a very narrow one, I am obliged to make use of a *funnel*, fig. 6, but by this means the operation is exceedingly easy; first filling the vessel into which the air is to be conveyed with water, and holding the mouth of it, together with the funnel, both under water with one hand, while the other is employed in pouring the air; which, ascending through the funnel up into the vessel, makes the water descend, and takes its place. These funnels are best made of glass, because the air being visible through them, the quantity of it may

be more easily estimated by the eye. It will be convenient to have several of these funnels of different sizes.

In order to expel air from solid substances by means of heat, I sometimes put them into a *gun-barrel*, Pl. II. fig. 7, and filling it up with dry sand, that has been well burned, so that no air can come from it, I lute to the open end the stem of a tobacco pipe, or a small glass tube. Then having put the closed end of the barrel, which contains the materials, into the fire, the generated air, issuing through the tube, may be received in a vessel of quicksilver, with its mouth immersed in a basin of the same, suspended all together by wires, in the manner described in the figure, or resting on a solid support: any other fluid substance may be used instead of quicksilver.

But the most accurate method of procuring air from several substances, by means of heat, is to put them, if they will bear it, into phials, such as *a, a, a*, Pl. IV. full of quicksilver, with their mouths immersed in the same, and then throwing the focus of a burning mirror upon them. For this purpose the phials should be made with their bottoms round, and very thin, that they may not be liable to break with a pretty sudden application of heat.

If I want to expel air from any liquid, I nearly fill a phial with it, and having a cork perforated, I put through it, and secure with cement, a glass tube

tube, bended in the manner represented at *e Pl. I. fig. 1.* I then put the phial into a kettle of water, which I set upon the fire and make to boil. The air expelled by the heat, from the liquor contained in the phial, issues through the tube, and is received in a bason of quicksilver. Instead of this suspended bason, I sometimes content myself with tying a flaccid bladder to the end of the tube, in both these processes, that it may receive the newly generated air.

I would observe, with respect to this process, and every other in which vessels are to be filled with quicksilver, and then to be placed inverted in basons of the same, that no operation is easier (unless the mouth of the vessel be exceedingly wide) when the mouth of it is covered with soft leather, and, if necessary, tied on with a string, before it be turned upside down; and the leather may be drawn from under it when it is plunged in the quicksilver. If the mouths of the vessels be very narrow, it will be sufficient, and most convenient, to cover them with the end of one's finger.

But if the air, disengaged from any substance, will be attracted by mercury, as is the case with all those which contain the nitrous acid, this process cannot be used, and recourse must be had to the vacuum; and for this purpose it is necessary that the operator be provided with receivers made very thin, on purpose for these experiments. Such as

are commonly used for other experiments are much too thick for this purpose, being very liable to break with the application of the heat produced by the burning lens. In this process, care should be taken to place the materials on glass, a piece of crucible, or some other substance that is known to yield no air by heat.

The figure, *b* Pl. IV. represents a common glass phial with a ground stopper, with many small holes in it, which was a happy contrivance of my ingenious pupil and friend Mr. Benjamin Vaughan. It is of excellent use to convey any liquid, or even any kind of air, contained in it, through the water, into a jar standing with its mouth inverted in it, without admitting any mixture of the common air, or even of the water; and yet the air generated within it has a sufficient out-let. These phials will be found useful in a great variety of experiments.

The figure *c*, represents a phial of the same form with *a*; but the neck is thicker, in order to be fitted with a ground stopper, perforated, and drawn out into a tube, to be used instead of the phial *e*, Pl. I. Till I hit upon this contrivance, which was executed for me by the direction of Mr. Parker, I had a great deal of trouble in perforating common corks, bending and fitting tubes to them; and, after all, the corks themselves, or the cement, with which I generally found it convenient to cover the ends of
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the tubes, were apt to give way, and to be the occasion of very disagreeable accidents. Besides, if any hot acid was used, the vapour would corrode the cork, and an allowance was to be made for the effect of that circumstance on the air: whereas, with this apparatus, which is exceedingly convenient and elegant, the operator may be sure that nothing but glass is contiguous to the materials he works upon, as he can perfectly exclude every other foreign influence; and while it remains unbroken, it is never out of repair, or unfit for use.

For many purposes, however, the former method, with corks and tubes, will be found very sufficient, and much less expensive; especially with the *fluor acid*, which corrodes glass, and which will presently eat through one of these delicate phials. For this purpose, therefore, I would recommend the use of a common and very thick phial, especially as no great degree of heat, and never any sudden application of heat, is wanted.

The phial *c*, will be found sufficient for any purpose that does not require more heat than the flame of a candle held close to the bottom of it, can supply: but if there be occasion to place the phial in a sand-heat, and consequently if it must be put into a crucible placed on the fire, it will be necessary to have the tube, in which the ground stopper terminates, made as long as may be, as represented by *e*;

otherwise the vessels that receive the air will be too near the fire. Nine or twelve inches, however, will be a sufficient length for any purpose.

I have great reason to congratulate myself on this apparatus, having found it to be of most admirable use. For, in experiments with air, where the greatest possible accuracy is required, *lutes* are by no means to be trusted, since a variety of vapours, coming into contact with them, are considerably affected; whereas these stoppers being ground airtight, the operator may be perfectly at ease, both with respect to the quantity and the quality of his produce. To express this process as concisely as possible, I generally allude to it, by saying that the phials have *ground stoppers and tubes*.

In experiments in which it is not worth while to be at the expence of these phials with ground stoppers and tubes, and yet where gun-barrels cannot be trusted to, on account of the materials corroding the iron, I have recourse to a kind of long phial, or a tube made narrower at the open end, nine or twelve inches in length, and of an equal thickness throughout, represented Pl. IV. fig. *d*. When these phials are put into a crucible with sand, the bottom may be made red-hot, while the top is so cool, that a common cork (into which a glass tube is inserted) will not be affected by the heat. In fact, this vessel is a kind of a gun-barrel made of glass, and is used

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exactly

exactly like the gun-barrel, except that it is not exposed to so great a degree of heat.

When the materials are put into this vessel, it must be filled up to the mouth with fine sand, that will give no air by the application of heat, and the cork must be thrust down close upon the sand. The air must be received as in plate 2. fig. 7. These glass vessels, however, will not bear a great degree of heat, and therefore by applying to Mr. Wedgwood (who is as great, and generous a friend of *science*, as he is distinguished by the wonderful improvements he has made on his own beautiful *art*) I got *earthen tubes* and *retorts*, which will bear any degree of heat, and being glazed, or not, as the occasion requires, I have found them of the most extensive use in my experiments.

When a person has a great many trials to make of the goodness of air, it is of no small importance to have contrivances by which he may save time. Having, particularly, had frequent occasion to measure the purity of air by means of nitrous air, in which it is sometimes necessary to put several measures of one kind to one measure of the other; and being wearied with taking all the measures separately, at length I hit upon the very useful expedient of having the measures ready made, consisting of vessels, the capacities of which had a known proportion to each other, as *f, f, f*, Pl. IV. each vessel holding twice as much as the size next less than it.

I found

I found it likewise convenient to have the vessels in which the mixture of air is made, fig. g, marked in a manner corresponding to these phials, that the diminution of the air may be perceived at once, without the application of any measure. If one of these phials contain an ounce-measure, and the rest be multiplies and subdivisions of it, it will be still more convenient.

There is a great variety of methods of mixing nitrous and common air, in order to ascertain the purity of the latter. But the manner in which I have now long been accustomed to perform that operation is still more simple, though it has nothing to boast of with respect to ingenuity. It is necessary to describe it, because it is referred to through the greater part of this work.

I first provide a phial, containing about an ounce of water, which I call *the air measure*. This I fill with air by having first filled it with water, and placed it over the opening of the funnel in my shelf; and when it is filled I slide it along the shelf, always observing that there be a little more air than I want. The phial being thus exactly filled with the air which I am about to examine, and care being taken that it be not warmed by holding in the hand, &c. I empty it into a jar about an inch and an half in diameter, and then introduce to it the same measure of nitrous air, and let them continue together about two minutes. I chuse to have an overplus of
nitrous

nitrous air, that I may be sure to have phlogiston enough to saturate all the common air. If I find the diminution with these measures to be very considerable, I introduce another measure of nitrous air; but the purest dephlogisticated air will not, I believe, require more than two equal measures of nitrous air.

Sometimes I leave the common and nitrous air in the jar all night, or a whole day; but always take care that, whatever kinds of air I be comparing together, they remain the same space of time before I proceed to note the degree of diminution. If the two kinds of air be agitated on coming into contact with each other, the diminution will be much greater; and therefore this circumstance should always be expressed.

When the preceding part of the process is over, I transfer the air into a glass tube, about two feet long, and one third of an inch wide, carefully graduated according to the air-measure, and divided into *tenths* and *hundred parts*; so that one of the latter will be about a sixth or an eighth of an inch. Then immersing the tube in a trough of water, so that the water in the inside of the tube shall be on a level with the water on the outside, I observe the space occupied by them both, and express the result in *measures*, and *decimal parts of a measure*, according to the graduation of the tube.

It is some trouble to graduate a tube in this manner; but when it is once done, the application of it

is extremely easy. As it will seldom happen that a glass tube is of an equal diameter throughout, I generally fill that part of the tube which contains one measure, with quicksilver, and then weighing it, and dividing it into ten parts, put them in separately, in order to mark the primary divisions. This operation is performed very readily by having a glass tube drawn out to a fine orifice, in order to take up a small quantity of quicksilver at a time, as it may be wanted.

Measuring the purity of respirable air, I mix with it an equal quantity of nitrous air, or if it be highly dephlogisticated, two equal quantities of nitrous air, which is always particularly mentioned in the course of this work: after this I transfer the mixture into a graduated tube. Consequently a less number in the result is always an indication of greater purity. This number, in order to be as concise as possible, I have in this work termed *the measure of the test*, or the *standard of the air*. Thus, if when I mix two equal quantities of common air and nitrous air, they afterwards occupy the space of one measure, and two tenths of a measure, I say *the measures of the test were 1. 2.* or the standard of the air was 1. 2.

If the quantity of the air, the goodness of which I wanted to ascertain, was exceedingly small, so as to be contained in a part of a glass tube, out of which water will not run spontaneously, I formerly

merly had recourse to the following method; I first measured with a pair of compasses the length of the column of air in the tube, the remaining part being filled with water, and laid it down upon a scale; and then, thrusting a wire of a proper thickness, into the tube, I contrived, by means of a thin plate of iron, bent to a sharp angle, to draw it out again, when the whole of this little apparatus was introduced through the water into a jar of nitrous air; and the wire being drawn out, the air from the jar must supply its place. I then measured the length of this column of nitrous air which I had got into the tube, and laid it also down upon the scale, so as to know the exact length of both the columns. After this, holding the tube under water, with a small wire I forced the two separate columns of air into contact; and when they have been a sufficient time together, I measured the length of the whole, and compared it with the length of both the columns taken before. But I now have tubes, made very small for this purpose, and a longer tube, graduated in proportion, which I use as I do the larger vessels when the quantity of air is sufficient.

In experiments on those kinds of air which are readily imbibed by water, I often make use of quicksilver, in the manner represented Pl. II. fig. 8, in which *a* is the basin of quicksilver, *b* a glass vessel
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containing quicksilver, with its mouth immersed in it, *c* a phial containing the ingredients from which the air is to be produced, and *d* is a small recipient, or glass vessel designed to receive and intercept any liquor that may be discharged along with the air, which is to be transmitted free from any moisture into the vessel *b*. If there be no apprehension of moisture, I make use of the glass tube only, without any recipient, in the manner represented *e* Pl. I. In order to invert the vessel *b*, I first fill it with quicksilver, and then carefully cover the mouth of it with a piece of soft leather; after which it may be turned upside down without any danger of admitting the air, and the leather may be withdrawn when it is plunged in the quicksilver.

In order to generate air by the solution of metals, or any process of a similar nature, I put the materials into a phial, prepared in the manner represented at *e* Pl. I. and put the end of the glass tube under the mouth of any vessel into which I want to convey the air. If heat be necessary I can easily apply to it a candle, or a red hot poker while it hangs in this position.

When I have occasion to transfer air from a jar standing in the trough of water to a vessel standing in quicksilver, or in any other situation whatever, I make use of the contrivance represented Pl. I. fig. 9, which consists of a bladder, furnished at one end with

with a small glass tube bended, and at the other with a cork, perforated so as just to admit the small end of a funnel. When the common air is carefully pressed out of this bladder, and the funnel is thrust tightly into the cork, it may be filled with any kind of air as easily as a glass jar; and then a string being tied above the cork in which the funnel is inserted, and the orifice in the other cork closed, by pressing the bladder against it, it may be carried to any place, and if the tube be carefully wiped, the air may be conveyed quite free from moisture through a body of quicksilver, or any thing else. A little practice will make this very useful manoeuvre perfectly easy and accurate. But I find it more convenient to have a small *brass cock*, to thrust into the cork, through which the air is introduced into the bladder.

In order to impregnate fluids with any kind of air, as water with fixed air, I fill a phial with the fluid, larger or less as I have occasion (as *a* Pl. I. fig. 10) and then, inverting it, place it with its mouth downwards, in a bowl *b*, containing a quantity of the same fluid; and having filled the bladder, fig. 9, with the air, I throw as much of it as I think proper into the phial, in the manner described above. To accelerate the impregnation, I lay my hand on the top of the phial, and shake it as much as I think proper.

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If, without having any air previously generated, I would convey it into the fluid immediately as it arises from the proper materials, I keep the same bladder in connexion with a phial *c* fig. 10, containing the same materials (as chalk, salt of tartar, or pearl ashes in diluted oil of vitriol, for the generation of fixed air) and taking care (lest, in the act of effervescence, any of the materials in the phial *c* should get into the vessel *a*) to place this phial on a stand lower than that on which the basin was placed, I press out the newly generated air, and make it ascend directly into the fluid. For this purpose, and that I may more conveniently shake the phial *c*, which is necessary in some processes, especially with chalk and oil of vitriol, I sometimes make use of a flexible leathern tube *d*, and sometimes only a glass tube. For if the bladder be of a sufficient length, it will give room for the agitation of the phial; or if not, it is easy to connect two bladders together by means of a perforated cork, to which they may both be fastened.

When I want to try whether any kind of air will admit a candle to burn in it, I make use of a cylindrical glass vessel, Pl. I. fig. 11, and a bit of wax candle *a* fig. 12, fastened to the end of a wire *b*, and turned up, in such a manner as to be let down into the vessel with the flame upwards. The vessel should be kept carefully covered till the moment that

that the candle is admitted. In this manner I have frequently extinguished a candle more than twenty times successively, in a vessel of this kind, though it is impossible to dip the candle into it without giving the external air an opportunity of mixing with the air in the inside more or less. The candle at the other end of the wire is very convenient for holding under a jar standing in water, in order to burn as long as the inclosed air can supply it; for the moment that it is extinguished, it may be drawn through the water, before any smoke can have mixed with the air.

In order to draw air out of a vessel which has its mouth immersed in water, and thereby to raise the water to whatever height may be necessary, it is very convenient to make use of a glass *syphon*, putting one of the legs up into the vessel, and drawing the air out at the other end by the mouth. If the air be of a noxious quality, it may be necessary to have a syringe fastened to the syphon, the manner of which needs no explanation. I have not thought it safe to depend upon a valve at the top of the vessel, which Dr. Hales sometimes made use of.

If, however, a very small hole be made at the top of a glass vessel, it may be filled to any height by holding it under water, while the air is issuing out at the hole, which may then be closed with wax or cement.

If the generated air will neither be absorbed by water, nor diminish common air, it may be convenient to put part of the materials into a cup, supported by a stand, and the other part into a small glass vessel, placed on the edge of it, as at *f* Pl. I. fig. 1. Then having, by means of a syphon, drawn the air to a convenient height, the small glass vessel may be easily pushed into the cup, by a wire introduced through the water; or it may be contrived, in a variety of ways, to discharge the contents of the small vessel into the larger. The distance between the boundary of air and water, before and after the operation, will shew the quantity of the generated air. The effect of processes that *diminish* air may also be tried by the same apparatus.

When I want to admit a particular kind of air to any thing that will not bear wetting, and yet cannot be conveniently put into a phial, and especially if it be in the form of a powder, and must be placed upon a stand (as in those experiments in which the focus of a burning mirror is to be thrown upon it) I first exhaust a receiver, in which it is previously placed; and having a glass tube, bended for the purpose, as in Pl. II. fig. 14, I screw it to the stem of a transfer of the air-pump on which the receiver had been exhausted, and introducing it through the water into a jar of that
kind

kind of air with which I would fill the receiver, I only turn the cock, and I gain my purpose. In this method, however, unless the pump be very good, and several contrivances, too minute to be particularly described, be made use of, a good deal of common air will get into the receiver.

In order to take the electric spark in a quantity of any kind of air, which must be very small, to produce a sensible effect upon it, in a short time, by means of a common machine, I put a piece of wire into the end of a small tube, and fasten it with hot cement, as in Pl. II. fig. 16; and having got the air I want into the tube, I place it inverted in a basin containing either quicksilver, or any other fluid substance by which I chuse to have the air confined. I then, by the help of the air-pump, drive out as much of the air as I think convenient, admitting the quicksilver, &c. to it, as at *a*, and putting a brass ball on the end of the wire, I take the sparks or shocks upon it, and thereby transmit them through the air to the liquor in the tube.

To take the electric sparks in any kind of fluid, as oil, &c. I use the same apparatus described above, and having poured into the tube as much of the fluid as I conjecture I can make the electric spark pass through, I fill the rest with quicksilver; and placing it inverted in a basin of quicksilver, I take the sparks as before.

If air be generated very fast by this process, I use a tube that is narrow at the top, and grows wider below, as fig. 17, that the quicksilver may not recede too soon beyond the striking distance.

Sometimes I have used a different apparatus for this purpose, represented fig. 18. Taking a pretty wide glass tube, hermetically sealed at the upper end, and open below; at about an inch, or at what distance I think convenient from the top, I get two holes made in it, opposite to each other. Through these I put two wires, and fastening them with warm cement, I fix them at what distance I please from each other. Between these wires I take the sparks, and the bubbles of air rise, as they are formed, to the top of the tube.

I have found it very convenient to have a number of *glass vessels*, such as represented Pl. V. fig. 2, for the purpose of making a quantity of air pass through a body of water, or any kind of fluid, or any substance in the form of powder; the air entering by the tube which goes to the bottom of the vessel, and being delivered by that which is inserted only at the top. I also found it necessary to have these vessels of various sizes, the largest containing about a pint, and the smallest about half an ounce measure of water. The larger end of this vessel I have generally closed with a cork, and cement; but I sometimes found it necessary to have this part also of glass,
with

with only two small perforations, for the insertion of glass tubes.

I have frequently had occasion to make use of a great number of these vessels at the same time, so disposed, as that the same air might pass through them all in succession, in the manner represented, fig. 3.

In some cases, however, I found it necessary to exclude all cement, and every kind of luting, from an apparatus of this kind; having had all the glass tubes fitted to their several holes by grinding. But this makes the apparatus very expensive, and especially the repairs of it.

Annexed to the last-mentioned apparatus, is a *long phial*, a fig. 3, with a tube fitted to it by grinding, and bent, so as to discharge the air, or vapour, issuing from it, downwards. This kind of phial I have generally used for my experiments with nitrous vapour. The phial is deep, in order to admit a sudden and violent effervescence without the danger of the liquor being thrown over, and the tube should be long enough, to go to the bottom of any vessel in which the vapour is to be delivered.

In distilling spirit of nitre, I have generally made use of the apparatus represented Pl. V. fig. 4, which was invented by Mr. Woulfe, consisting of a retort *a*, an adopter, if necessary, *b*, and a receiver *c*, with two orifices; one *d*, for the discharge of the distilled acid,

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and

and the other *e*, to serve as an outlet for the superabundant vapour; which, passing through the glass tube *f*, may impregnate the water in the basin *g*.

Pl. III. fig. 4, represents a *cylindrical vessel* made of tin, inclosing another of iron wire. In the outer vessel a charcoal fire may be made, surrounding the inner cylinder, which, being open at the bottom, will admit the upper part of a glass jar, supported in whatever manner the operator may find most convenient. Thus a jar, with the air, &c. contained in it, may be heated as much as the glass will bear, without giving more heat than is necessary to the lower part of it. In this manner also, an equal degree of heat may be given to every side of the upper part of the glass.

Pl. III. fig. 5, explains the manner in which I make an electrical explosion pass through any substance in the form of vapour. It represents a glass syphon, in each leg of which is an iron wire, of such a length, that there shall only be about half an inch between the heads of them. The syphon must be filled with mercury, and each of the legs inserted in separate basins, also containing mercury. After this, the substance may be introduced into the syphon by means of a glass tube, and, being lighter, it will take its place in the bend of the syphon; which may then be placed near the opening of a small furnace, or in the apparatus described fig. 3, when whatever

ever lodges in the upper part of the syphon will be converted into vapour, and the explosion will be made in it by making the syphon part of an electrical circuit. Mercury itself may be converted into vapour in the same manner.

It may be worth while to give a short account of the *earthen jar*, in which I made many of the experiments on the growth of plants in different kinds of air, recited in this volume; and a bare inspection of Pl. VI. fig. 1, will be almost sufficient for this purpose.

The jar was about eighteen inches in diameter at the top, and of the same depth. It was placed in an open exposure in the garden, and sticks were thrust into the earth in a perpendicular position, quite round it; and to these sticks glass jars, filled with water, with their mouths inverted in the water of the earthen jar, were fastened by strings. After I had introduced into one of these jars any particular kind of air, I afterwards drew through the water, and put into it, any plant, the top and leaves of which I wished to expose to it; supporting the root or stalk at a proper height in the earthen jar, if I found that any such support was necessary. In some cases it will be found that the top of the plant was in one jar, and the root or stalk in another; which it was not at all difficult to do.

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Fig.

Pl. VI. fig. 2, represents the instrument by which I endeavoured to ascertain the conducting power of different kinds of air with respect to heat. It consists of a glass bulb open at both ends, so that I could easily fasten a thermometer with its bulb in the center of it, where it would be surrounded by any kind of air, introduced into it after it had been previously filled with mercury. The manner in which the experiments were made is sufficiently described in the account of them.

Pl. VII. fig. 1, is a view of the apparatus with which the principal experiments relating to the seeming conversion of water into air were made. It consists of an *earthen vessel*, the bulb of which, containing moistened clay, is fixed in the inside of a *glass vessel*, through which the heat of a burning lens may be thrown upon it; while the inside has a communication with a basin of water, or mercury, in which vessels may be placed to receive the air that is forced through the body of the earthen vessel; while the water, or mercury, in the *basin* in which the glass vessel stands, rises within it, to supply the place of that air.

Pl. VII. fig. 2, shews the disposition of the apparatus by which steam is transmitted through a red-hot tube, containing iron, &c. with a *worm tub* to collect the superfluous water, &c. and a vessel to receive the
air

air that is produced. This vessel is here drawn very small, that it might not take up much room in the plate; but I have generally used a large trough for this purpose, and jars of considerable size to receive the air. Instead of the small *furnace* to heat the water, &c. I now use one of *Mr. Argand's lamps*, which is, on several accounts, a very valuable addition to a chemical apparatus. Fig. 6, represents the method of receiving the air in this process under a funnel, fixed in a trough of water, which may be used when large balloons are filled, and when no account is taken of any water that is condensed in the process.

Fig. 4, represents a large glass balloon, in which inflammable air, issuing from the orifice of a small tube, burns like a candle, while the water produced by the process is collected in the inside of it.

Fig. 5, represents a strong cylindrical glass vessel, in which inflammable and dephlogisticated air may be fired. It is furnished with a wooden cap, firmly cemented to the open end of it, and closed with a screw, and two iron wires are inserted at the top of it, between which an electric spark can be taken.

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THE weights mentioned in the course of this treatise are Troy, and what is called an ounce measure of air, is the space occupied by an ounce weight of water, which is equal to 480 grains, and is, therefore, almost two cubic inches of water; for one cubic inch weighs 254 grains. Having sometimes used the penny-weight, it may be necessary to acquaint Foreigners, that 24 grains are a penny-weight, that 20 of such penny-weights make an ounce, and 12 ounces a pound.

The same ounce Troy, is, by Apothecaries, divided into eight drams, each dram into three scruples, and the scruple into twenty grains.

B O O K I.

OBSERVATIONS AND EXPERIMENTS RELATING TO FIXED AIR.

P A R T I.

OF THE RELATION OF FIXED AIR TO WATER.

SECTION I.

Of the impregnation of water with fixed air.

IT was in consequence of living for some time in the neighbourhood of a public brewery, a little after Midsummer in 1767, that I was induced to make experiments on fixed air, of which there is always a large body, ready formed, on the surface of the fermenting liquor, generally about nine inches, or a foot, in depth, within which any kind of substance may be very conveniently placed; and though, in these circumstances, the fixed air must be continually mixing with the common air, and is therefore

fore far from being perfectly pure, yet there is a constant fresh supply from the fermenting liquor, and it is pure enough for many purposes.

A person, who is quite a stranger to the properties of this kind of air, would be agreeably amused with extinguishing lighted candles, or chips of wood in it, as it lies upon the surface of the fermenting liquor. For the smoke readily unites with this kind of air, probably by means of the water which it contains; so that very little or none of the smoke will escape into the open air, which is incumbent upon it. It is remarkable, that the upper surface of this smoke, floating in the fixed air, is smooth, and well defined; whereas the lower surface is exceedingly ragged, several parts hanging down to a considerable distance within the body of the fixed air, and sometimes in the form of balls, connected to the upper stratum by slender threads, as if they were suspended. The smoke is also apt to form itself into broad flakes, parallel to the surface of the liquor, and at different distances from it, exactly like clouds. These appearances will sometimes continue above an hour, with very little variation. When this fixed air is very strong, the smoke of a small quantity of gunpowder fired in it will be wholly retained by it, no part escaping into the common air.

Making an agitation in this air, the surface of it (which still continues to be exactly defined) is thrown
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into the form of waves, which is very amusing to look upon; and if, by this agitation, any of the fixed air be thrown over the side of the vessel, the smoke, which is mixed with it, will fall to the ground, as if it was so much water, the fixed air being heavier than common air.

Fixed air does not instantly mix with common air. Indeed if it did, it could not be caught upon the surface of the fermenting liquor. A candle put under a large receiver, and immediately plunged very deep below the surface of the fixed air, will burn some time. But vessels with the smallest orifices, hanging with their mouths downwards in the fixed air, will *in time* have the common air, which they contain, perfectly mixed with it. When the fermenting liquor is contained in vessels close covered up, the fixed air, on removing the cover, readily affects the common air which is contiguous to it; so that, candles held at a considerable distance above the surface will instantly go out. I have been told by the workmen, that this will sometimes be the case, when the candles are held two feet above the mouth of the vessel.

Fixed air unites with the smoke of rosin, sulphur, and other electrical substances, as well as with the vapour of water.

I also held some oil of vitriol in a glass vessel within the fixed air, and by plunging a piece of red-hot glass into it, raised a copious and thick fume.

This floated upon the surface of the fixed air like other fumes, and continued as long.

Considering the near affinity between water and fixed air, I concluded that if a quantity of water was placed near the yeast of the fermenting liquor, it could not fail to imbibe that air, and thereby acquire the principal properties of Pyrmont, and some other medicinal mineral waters. Accordingly, I found, that when the surface of the water was considerable, it always acquired the pleasant acidulous taste that Pyrmont water has. The readiest way of impregnating water with this virtue, in these circumstances, is to take two vessels, and to keep pouring the water from one into the other, when they are both of them held as near the yeast as possible; for by this means a great quantity of surface is exposed to the air, and the surface is also continually changing. In this manner, I have sometimes, in the space of two or three minutes, made a glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont, or rather Seltzer water.

One would naturally think, that having actually impregnated common water with fixed air, produced in a brewery, I should immediately have set about doing the same thing with air let loose from chalk, &c. by some of the stronger acids. But, easy as the practice proved to be, no method of
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doing it at that time occurred to me. I still continued to make my Pymont water in the manner above mentioned till I left that situation, which was about the end of the summer 1768; and from that time, being engaged in other similar pursuits, I made no more of the Pymont water till the spring of the year 1772.

In the mean time I had acquainted all my friends with what I had done, and frequently expressed my wishes that persons who had the care of large *distilleries* (where I was told that fermentation was much stronger than in common breweries) would contrive to have vessels of water suspended within the fixed air which they produced, with a farther contrivance for agitating the surface of the water; as I did not doubt but that, by this means, they might, with little or no expence, make great quantities of Pymont water; by which they might at the same time both serve the public, and benefit themselves. For I never had the most distant thought of making any advantage of the scheme myself.

In all this time, viz. from 1767 to 1772, I never heard of any method of impregnating water with fixed air but that above mentioned. My thinking at all of reducing to practice any method of effecting this, by air dislodged from chalk, and other calcaræous substances, was occasioned by my hearing of Dr. Irving's method of distilling sea water
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for the use of the navy. For it occurred to me, that if seamen could be taught a method of impregnating that or any other water with fixed air, it might be farther useful to prevent, or to cure the sea scurvy, going upon Dr. Macbride's idea of fixed air being an antiseptic.

Mentioning this scheme to Sir George Saville, he introduced me to Lord Sandwich, then at the head of the admiralty, who procured an order for the college of physicians to examine it. As they were pleased to recommend the trial of it, I drew up an account of the method which I had then devised, in a small pamphlet; the substance of which, as it is no longer published separately, I insert here.

Directions for impregnating water with fixed air.

If water be only in contact with fixed air, it will begin to imbibe it, but the mixture is greatly accelerated by agitation, which is continually bringing fresh particles of air and water into contact. All that is necessary, therefore, to make this process expeditious and effectual, is first to procure a sufficient quantity of this fixed air, and then to contrive a method by which the air and water may be strongly agitated in the same vessel, without any danger of admitting the common air to them; and this is easily done by first filling any vessel with water, and introducing
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the fixed air to it, while it stands inverted in another vessel of water.

Take therefore a glass vessel, *a*, Pl. VIII. fig. 1. with a pretty narrow neck, but so formed, that it will stand upright with its mouth downwards (or it may be supported as in Pl. III. fig. 3) and having filled it with water, lay a slip of clean paper, or thin pasteboard upon it. Then, if they be pressed close together, the vessel may be turned upside down, without danger of admitting common air into it; and when it is thus inverted, it must be placed in another vessel, in the form of a bowl or basin, *b*, with a little water in it, so much as to permit the slip of paper or pasteboard to be withdrawn, and the end of the pipe *c* to be introduced.

This pipe must be flexible, and air-tight, for which purpose it is, I believe, best made of leather, sewed with a waxed thread, in the manner used by shoemakers. Into each end of this pipe a piece of a quill, or tube of tinned iron, should be thrust, to keep them open, while one of them is introduced into the vessel of water, and the other into a cork, which must be perforated, and fitted to a vessel *e*, two thirds of which should be filled with chalk, or pounded marble, well covered with water.

Things being thus prepared, and the vessel containing the chalk and water being detached from the vessel of water, pour a little oil of vitriol upon the chalk and water, and put the cork into the

bottle a little time after the effervescence has begun; and then introduce the end of the pipe into the mouth of the vessel of water, as in the drawing, and, if necessary, agitate the chalk and water briskly. This will presently produce a considerable quantity of fixed air, which will force its way through the pipe, and ascend into the vessel of water, the water at the same time descending, and coming into the basin.

When about one half of the water is forced out, let the operator lay his hand upon the uppermost part of the vessel, and shake it as briskly as he can, and in a few minutes the water will absorb the air; and taking its place, will nearly fill the vessel as at the first. Then shake the phial containing the chalk and water again, and force more air into the vessel, till, upon the whole, about an equal bulk of air has been thrown into it. Also shake the water as before, till no more of the air can be imbibed. As soon as this is perceived to be the case, the water is ready for use; and if it be not used immediately, should be put into a bottle as soon as possible, well corked, and cemented. It will keep, however, very well, if the bottle be only well corked, and kept with the mouth downwards.

In general, the whole process does not take up more than about a quarter of an hour, the agitation not five minutes; and in nearly the same time might a vessel of water, containing two or three gallons,

gallons, or indeed any quantity that a person could well shake, be impregnated with fixed air, if the phial containing the chalk and oil of vitriol, be larger in the same proportion.

To give the water as much air as it can receive in this way, the process may be repeated with the water thus impregnated. I generally chuse to do it two or three times, but very little will be gained by repeating it oftener; since, after some time, as much fixed air will escape from that part of the surface of the water which is exposed to the common air, as can be imbibed from within the vessel.

The pressure of the atmosphere assists very considerably in keeping fixed air confined in water; for in an exhausted receiver, Pyrmont water will absolutely boil, by the copious discharge of its air. This is also the reason why beer and ale froth so much *in vacuo*. I do not doubt, therefore, but that, by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring; and it would not be difficult to contrive a method of doing it.

All calcareous substances contain fixed air, and any acids may be used in order to set it loose from them; but pounded lime stone, or the sawings of marble, and oil of vitriol are, both of them the cheapest, and, upon the whole, the best for the purpose.

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I should think that there can be no doubt, but that water thus impregnated with fixed air must have all the medicinal virtues of genuine Pyrmont or Seltzer water; since these depend upon the fixed air they contain. If the genuine Pyrmont water derives any advantage from its being a natural chalybeate, this may also be obtained by providing a common chalybeate water, and using it in these processes, instead of common air.

If any person would chuse to make this medicated water more nearly to resemble genuine Pyrmont water, Sir John Pringle informs me, that from eight to ten drops of *Tinctura Martis cum spiritu salis* must be mixed with every pint of it. It is agreed, however, on all hands, that the peculiar virtues of Pyrmont, or any other mineral water which has the same brisk or acidulous taste, depend not upon its being a chalybeate, but upon the fixed air which it contains.

But water impregnated with fixed air does of itself dissolve iron, as the ingenious Mr. Lane has discovered; and iron filings put to this medicated water make a strong and agreeable chalybeate, similar to some other natural chalybeates, which hold the iron in solution by means of fixed air only, and not by means of any acid; and these chalybeates, I am informed, are generally the most agreeable to the stomach.

By this process may fixed air be given to wine, beer, and almost any liquor whatever: and when beer is become flat or dead, it will be revived by this means; but the delicate agreeable flavour, or acidulous taste communicated by the fixed air, and which is manifest in water, will hardly be perceived in wine, or other liquors which have much taste of their own.

I would not interfere with the province of the physician, but I cannot intirely satisfy myself without taking this opportunity to suggest such hints as have occurred to myself, or my friends, with respect to the *medicinal uses* of water impregnated with fixed air, and also of fixed air in other applications.

In general, the diseases in which water impregnated with fixed air will most probably be serviceable, are those of a *putrid* nature, of which kind is the *sea-scurvy*. It can hardly be doubted, also, but that this water must have all the medicinal virtues of Pyrmont water, and of other mineral waters similar to it, whatever they be; especially if a few iron filings be put to it, to render it a chalybeate, like genuine Pyrmont water. It is possible, however, that, in some cases, it may be desirable to have the *fixed air* of Pyrmont water, without the *iron* which it contains.

Having this opportunity, I shall also hint the application of fixed air in the form of *clysters*, which occurred to me while I was attending to

this subject, as what promises to be useful to correct putrefaction in the intestinal canal, and other parts of the system to which it may, by this channel, be conveyed. It has been tried once by Mr. Hey, of Leeds, and the recovery of the patient from an alarming putrid fever, when the stools were become black, hot, and very fetid, was so circumstanced, that it is not improbable but that it might be owing, in some measure, to those clysters. The application, however, appeared to be perfectly easy and safe. Also Dr. Warren, of Taunton, administered fixed air in the same manner, with the most happy effect.

I cannot help thinking that fixed air might be applied externally to good advantage in other cases of a putrid nature, even when the whole system was affected. There would be no difficulty in placing the body so, that the greatest part of its surface should be exposed to this kind of air; and if a piece of putrid flesh will become firm and sweet in that situation, as Dr. Macbride found, some advantage, I should think, might be expected from the same antiseptic application, assisted by the *vis vita*, operating internally, to counteract the same putrid tendency. Some Indians, I have been informed, bury their patients, labouring under putrid diseases, up to the chin in fresh mould, which is also known to take off the fætor from flesh meat beginning to putrify. If this practice be of any use,

use, may it not be owing to the fixed air imbibed by the pores of the skin in that situation?

Being no physician, I run no risque by throwing out these random hints and conjectures. I shall think myself happy, if any of them should be the means of making those persons, whom they immediately concern, attend more particularly to the subject.

There is another ingenious method of impregnating water with fixed air, contrived by Dr. Nöoth, by means of three glass vessels, as represented in Pl. IX.

In the lowest vessel, the chalk or marble, and the water acidulated with oil of vitriol, must be put, and into the middle vessel the water to be impregnated. During the effervescence, the fixed air rises into the middle vessel, and rests upon the surface of the water in it, while the water that is displaced by the air rises through the bent tube into the uppermost vessel, the common air going out through the channel in the stopper. When the bent tube is of a proper length, the process requires no attention; and if the production of air be copious, the water will generally be sufficiently impregnated in five or six hours. At least, all the attention that needs be given to it is to raise the uppermost vessel once or twice, to let out that part of the fixed air which is not readily absorbed by water. If the operator chuse to accelerate the process, by agitating the water, he must separate

the two uppermost vessels from the lowest. For if he should agitate them all together, he will occasion too copious a production of air; and he will also be in danger of throwing the liquor contained in the lowest vessel into contact with the stopper which separates it from the middle vessel, by which means some of the oil of vitriol might get into the water.

SECTION II.

Of the State of Air in Water.

AFTER treating of the impregnation of water with fixed air, I shall recite the observations I have at different times made on the state of air expelled from water by heat, especially as in several cases this is fixed air.

I have frequently found air expelled from water to be much better than common air; but I have not yet undertaken any regular course of experiments on the subject; such as examining the same water at different times of the year, with different impregnations, different exposures, &c. which I wish to have done; because I think it possible, that something worth know-

knowing relating to the properties of water, or of air in water, especially respecting phlogiston, and the general state of the atmosphere, may be discovered by this means. Such observations as I have occasionally made I shall here put down.

Boiling generally expels more or less of fixed air from water. On the 5th of June, 1779, I found my pump water to yield air, one fifth of which was fixed air, and the measures of the test for the residuum were 1. 5.* The same pump water, which had been boiled some time before, gave air, one seventh of which was fixed air, and the measures of the test for the residuum were 1. 4. In general I believe a greater difference than this will be found in these two cases. I do not know that water will attract fixed air from the atmosphere, at least in the proportion in which it is generally found in pump water, which is probably acquired from calcareous matters first held in solution, and then partially decomposed in it.

Water distilled in a glass, which had been long exposed to the open air, yielded air, of which little or none was fixed air, and with equal quantities of nitrous air, the measures of the test were 1. 1.

A quantity of rain water taken from a large tub, which had long stood exposed to the open air, yield-

* In the experiments mentioned in this book, the two kinds of air were not agitated when they were mixed.

ed one fixtieth of its bulk of air, of which no part was fixed air, and the meafures of the test were 1.4. Perhaps the wood of the tub, or some other matter casually falling into it, might have contaminated this air.

A quantity of river water, not very far from the spring, gave one fiftieth of its bulk of air of which the smallest part imaginable was fixed air, and the meafures of the test were 1.05. This air was very pure; but the part of the river from which I took it was nearly stagnant, and very full of water plants.

Lime water is certain not to contain any fixed air. From a quantity of this water I expelled air so pure that the meafures of the test were 1.0. The quantity of air was one fiftieth of its bulk. Upon the whole I am inclined to infer, from all the observations I have hitherto made, that this is about the standard of air contained in water, which has no fixed air, and has been exposed to no influences except those of the common atmosphere, in its usual state.

From a spring which was remarkable for its petrifying quality, I expected much fixed air, but I found none; and the air I extracted from it was a little worse than common air. It is plain that, in this case, a boiling heat had not decomposed the lime stone it contained.

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I also filled a phial with pump water and pounded lime stone, exposed to the sun from the 28th of May to the 3d of July, when it yielded air so pure, that with two equal quantities of nitrous air, the measures of the test were 1.04. I should have suspected some green vegetable matter in this water, but I could not perceive any. Perhaps some latent, or nascent vegetation, might be the cause of this very pure air.

That water imbibes dephlogisticated air from the atmosphere, is evident from the following observation. I took some of the Bristol water in which fishes had died, and which then yielded air thoroughly phlogisticated; and having exposed it to the sun from the 28th of May to the 3d of July, I found it to yield a considerable quantity of air; and so pure that, with an equal quantity of nitrous air, the measures of the test were 0.76, and with two equal quantities of nitrous air the measures were 1.18.

Fixed air abounds so much in some mineral waters, that their peculiar virtues are certainly owing to this ingredient in their composition. This consideration has led some persons to ascribe the virtues of other mineral waters to this principle, though they contain it in so very small a proportion, as to make that opinion very improbable. Some, for instance, have thought that the virtues of the *Bath-water*

water were owing, in a great measure, to the fixed air it contains ; and living at no great distance from that celebrated spring, I thought I should incur a just censure, if I did not endeavour to ascertain what kind of air is contained in that water, and in what proportion. Accordingly, I made an excursion as far as Bath, chiefly with that view, and made the following experiments, which, having no apparatus of my own along with me, I was enabled to perform by the friendly zeal and ingenuity of Mr. Painter ; Dr. Gushart, Dr. Falconer, and Dr. Watson, favouring me with their presence.

In order to ascertain what proportion of air is contained in the water, in the state in which it is drank, I filled a pint-phial with the water hot from the pump, and expelled the air from it, by boiling it about four hours, receiving the produce in quicksilver. This air was about one thirtieth of the bulk of the water, and about one half of it was fixed air, precipitating lime in lime-water, and being readily absorbed by water. The residuum appeared, by the test of nitrous air, to be rather better than air in which a candle had burned out.

The quantity of fixed air that appears, by this experiment, to be contained in the Bath-waters is so very small, that I think it very improbable that their virtues should be at all owing to it. Few spring-waters, I believe, contain much less fixed air,
and

and many I know, which have no medicinal virtue at all, contain more. The pump-water belonging to the house in which I lived at Calne, contains about one fourteenth of its bulk of fixed air; and my pump-water at Leeds, contained about one fiftieth of its bulk of air, of the very same composition as the air of the Bath-waters, viz. half of it fixed air, and half common air, a little phlogisticated, so as to be in about the same state as air in which a candle had burned out.

Besides, the length of time which the Bath-waters, and indeed most other spring-waters, require to expel the air by means of heat, shews that the air expelled from them, was not contained in them in that state in which it is contained in waters properly impregnated with fixed air, out of which it may always be expelled by the heat of boiling water in less than an hour. In fact, the fixed air is not united to the *water*, but to some *calcareous matter* in the water, out of which the air is expelled with much more difficulty. Accordingly, Dr. Falconer informs me, that there is a deposit made by this water, after long boiling. If so, it may be presumed, that these waters do not so properly contain fixed air, as a calcareous earth; which, though it contain fixed air, may not part with it in the stomach, unless it meet with some acid to decompose it.

Besides

Besides the air contained in the Bath-water, there is a considerable quantity of air continually bubbling up from almost every part of the soil, through the water in the bath. To examine this, I took about a pint of that air, and found, upon examination, that only about one twentieth of its bulk was fixed air, precipitating lime in lime-water, and being readily absorbed by water. The rest extinguished a candle, and was so far phlogisticated, that two measures of it, and one of nitrous air, occupied the space of $2\frac{1}{2}$ of a measure ; that is, it was almost perfectly noxious.

Being in Germany in the summer of the year 1774, we happened to pass by the famous spring of Seltzer-water, near Schwallbach, and also another very hot spring near the road from that place to Mentz. Through both these springs there was a bubbling of air, exactly similar to that in the Bath-waters ; but I had not time, or convenience, for making the same experiments upon them, and therefore contented myself with finding that the air of both of them extinguished a candle.

P A R T II.

OF THE SUBSTANCES WHICH YIELD FIXED AIR
CHIEFLY BY HEAT.

S E C T I O N I.

Of Air extracted from mineral Substances.

HAVING in an early period of my experiments, found that manganese, and other natural mineral substances, yield a very pure air by extreme heat; it occurred to me that subterraneous fires might maintain themselves by means of the air which they dislodged from such substances as they found in the bowels of the earth. This led me to try what *kind*, and what *quantity* of air, would be yielded by various mineral substances, in great heats, and it may not be useless to recite the experiments, as a knowledge of the results may be useful in other philosophical inquiries; and as many of them yielded fixed air, I shall insert the account of them in this place.

As the original object of my inquiry respected *volcanic fires*, I gave particular attention to the examination

mination of volcanic substances, especially with a view to ascertain whether a substance which had been in a state of *fusion* will yield air by being heated again or not; in order to distinguish the products of volcanos from other stony matters. Though charcoal, which has been exposed to the most intense heat, will imbibe air from the atmosphere, and give it out on being heated a second time, yet this is not a substance that can be *fused*; and as this does not appear to be the property of earthy substances, some dependence may perhaps be placed on this test. If so, *basaltes* can hardly be classed among volcanic productions, because they yield more air by heat than any known *lava* that I have met with.

But Mr. Keir has observed to me, that a substance from which air has been expelled by fusion, may yield more air by being melted again in a greater degree of heat, so that this test is not decisive.

As the results of the experiments that I made both with lavas and basaltes were various, I shall briefly cite them.

Of lava from Iceland, four ounces and one fifth, heated in an earthen retort, gave twenty ounce measures of air, of which one half, towards the beginning of the process, was fixed air, and the remainder of the standard of 1.72, extinguishing a candle. In the interstices of this lava, there was a brownish sand, which I could not separate from it.

Of

Of lava from Vesuvius, five ounces and a half, yielded thirty ounce measures of air, of which the first portion had a slight appearance of fixed air, and the rest was phlogisticated, from the standard of 1.64, to 1.38, which came last. The retort was broken by the swelling of the mass in cooling.

Another ounce of lava, of the consistence of a hard stone, yielded only three ounce and a half measures of air, chiefly inflammable, which, I suppose, came from the gun-barrel in which this particular experiment was made.

From these experiments it seems probable, that genuine lavas do not give much air; but this will depend upon the degree of heat to which they have been subjected in the subterraneous fire.

It has been much disputed whether basaltcs be a volcanic production, or only a crystallization of a mass of matter in a fluid form. The following experiments incline me to the latter opinion.

Seven ounces of basaltcs from Scotland, heated in an earthen retort, yielded 104 ounce measures of air, of which the first portion had a slight appearance of fixed air, and was so much phlogisticated as to extinguish a candle, being sometimes of the standard of 1.68.

About two ounces of the *giants causeway* in Ireland, yielded forty ounce measures of air, the first portion of which had a slight appearance of fixed air,

and the rest phlogisticated, of the standard of 1.65. It was reduced by fusion to a hard black glass.

Of basalt from Scotland, five ounces one hundred and sixty-two grains, yielded seventy-eight ounce measures of air, of which no part appeared to be fixed air; but was all phlogisticated, sometimes of the standard of 1.7, and towards the last 1.41.

The neighbourhood of Birmingham abounds with a stone which, from its being chiefly got from a village called Rowley, near Dudley, is commonly called *Rowley-rag*. When it is broken, it very much resembles basalt, though it is not found in the same regular form. Dr. WITHERING, of this place, has given a most excellent analysis of this substance, which may be seen in a late volume of the Philosophical Transactions. All that I did with respect to it was, to subject it to a strong heat in an earthen retort; and from this mode of examination it should seem to be of the same nature with the basalt, whatever that be.

Four ounces of the Rowley-rag yielded forty ounce measures of air, containing hardly any appearance of fixed air, but was phlogisticated, of the standard of 1.6, and 1.5; the last portion 1.31. It was reduced to a black glassy substance, which broke with a polish, exactly resembling that which remained from the basalt.

The

The Derbyshire *toadstone*, in its appearance, very much resembles the Rowley-rag, excepting that it is full of white spots, consisting of a calcareous substance. Dr. WITHERING has analysed this, as well as the Rowley-rag, and both from his experiments and mine, they seem to be nearly a-kin to each other. Two ounces and 384 grains of this substance, from which the calcareous part had been dissolved by spirit of nitre, yielded sixty ounce measures of air, the first portion of which contained a little fixed air, perhaps from some unperceived remains of the calcareous matter. The rest was phlogisticated, of the standard of 1.7.

In another experiment, an ounce and a quarter of this substance, from which the calcareous part had been extracted first by oil of vitriol, and then by spirit of nitre, yielded 40 ounce measures of air, of the same quality of that in the former process. There remained from both of them a black glassy matter, which seemed to be very liquid when it was hot, as part of it had boiled up into the neck of the retort.

Granite, like basaltes, has been thought by some to be the product of volcanoes, and by others to be a crystallization from a liquid state. The latter is the opinion favoured (but for the reason given above not decisively proved) by these experiments. From about an ounce and an half of this sub-

stance I got twenty ounce measures of air, the first portion of which contained a little fixed air, but the rest was phlogisticated, from 1.7 to 1.28, which is nearly the standard of common air; but the heat was very intense, as the substance was reduced to a glass.

Again, five ounces and 252 grains of a blue granite yielded seventy ounce measures of air, of the same quality with the preceding. It was also reduced to a firm uniform glassy substance, of a dark-brown colour. Upon the whole, therefore, it seems probable, that the origin of granite is similar to that of basalt.

In Cornwall there is a substance called *elvain*, the natural history of which is very like that of granite, and the result of my experiments upon it shews that they are of the same nature. There is a black and white kind of *elvain*.

Of the black *elvain* one ounce and 288 grains yielded twenty-five ounce measures of air, the first portion of which contained a little fixed air, and the rest was phlogisticated, of the standard of 1.54. It was melted into a brownish black mass.

Of the white *elvain* one ounce and 384 grains yielded thirty ounce measures of air, of the same quality with the preceding. It was converted into a very porous substance, exactly resembling a pumice stone, but much harder.

The

The substance called *groan clay* is said to be formed by the decomposition of granite. Of this substance one ounce and seventy-two grains yielded thirty-two ounce measures of air, containing no sensible quantity of fixed air, but all phlogisticated, of the standard of 1.62 and 1.33. After the experiment this matter was easily shaken out of the retort, and was not sensibly changed in its appearance.

Such were the experiments that I made with substances that are, or are supposed to be, volcanic. Of those which are certainly *not volcanic*, but which may come in the way of volcanic fires, I found those into which the vitriolic acid enters to yield the greatest quantity of pure air; but by no means sufficient to keep alive such fires as we make on the surface of the earth.

From seven ounces of *gypsum*, which I kept in a strong heat twelve hours, I got 230 ounce measures of air, the greatest part of which would have extinguished a candle; the most phlogisticated being of the standard of 1.8, but it was afterwards much purer; and at the last considerably dephlogisticated; for with two equal measures of nitrous air, the test was 1.3. The air was very turbid as it was produced, and the purest of all came rapidly, at the end of the process. It is possible

that, with a stronger heat, more, and purer air might have been procured. The substance was reduced to a *hard mass*, yellow next to the retort, but in the middle very white.

The *stones* which I found to furnish the greatest quantity of air, though not the purest, were those of the *schistus* kind, which are found in great quantities in many mountainous countries; and after being subjected to a very great heat, have the nearest resemblance to the generality of lavas of any substance on which I have yet made the experiment.

From four ounces of a *blue slate* I got 320 ounce measures of air, a very small portion of which was fixed air, and the greatest part of the rest (the whole, I believe, except about twenty ounce measures) so impure, that the standard was generally 1.8. Towards the last it was 1.5, and the last of all 1.35; so that a candle would just have burned in it. The air was very turbid, and had a very strong smell. The substance was perfectly vitrified, and quite black, exactly resembling lava. It then weighed, as nearly as I could guess (for in the fusion it had adhered closely to the retort) three ounces and 288 grains.

From eight ounces of another kind of *schistus*, I got seventy ounce measures of air, of the same
quality

quality with that in the preceding experiment; and it was melted into a black mass, harder than the former, so as to make a still more perfect lava.

Oil of vitriol has been supposed to enter into the composition of *clay*. From four ounces of it I got twenty ounce measures of air, in every portion of which one-tenth was fixed air, and the rest of the standard of 1.72, 1.52, and at last 1.44.

Putting oil of vitriol to this clay, it yielded much more air, and of a better quality. Two ounces of the clay, moistened with this acid, gave 210 ounce measures of air, exceedingly turbid, containing very little fixed air, and the rest of the standard of 1.5, 1.7, 1.58, in the order in which they are here put down; but the last portion was 1.08, and was considerably dephlogisticated.

A quantity of fine white *clay from the Apallachian mountains* gave air of the same kind at the beginning of the process with common clay, but the retort being cracked, the experiment was interrupted.

Nothing in the form of a *stone* yields so much air as *lime stone*, and this is by no means all fixed air, as I believe has generally been supposed. For a very great proportion of it is more or less phlogisticated, and the last portions often tolerably pure, so that a candle would nearly burn in it.

From four ounces of white *crystals of lime stone* I got 830 ounce measures of air, the first portion of which had only one-fourth of fixed air, but in the course of the experiment it varied, being once three-fourths, then one-half, and at the last one-third. The standard of the residuum was never better than 1.56, nor worse than 1.66.

From five ounces and a half of lime stone of an excellent kind, I got in all 1160 ounce measures of air. Of this one-tenth only was phlogisticated, and the rest fixed, but the last portion of all was half phlogisticated.

From seven ounces of a transparent substance, found in a stone in the neighbourhood of Oxford, which is chiefly calcareous, I got 1280 ounce measures of air, of which about one-third of the whole was fixed air. The standard of the residuum was at first 1.55, and afterwards 1.44.

From six ounces of a *blue stone*, found in the neighbourhood of Stratford, I got 1030 ounce measures of air, of which, till near the end of the process, about one half was fixed air, and at the last about one fourth. The standard of the remainder was about 1.6.

From three ounces of *chalk* I got 630 ounce measures of air, of which at the first one fourth was fixed air, then almost two-thirds, then something

thing more than one half, and again a little more than a third. The standard of the residuum was from 1.66 to 1.34.

The purest calcareous earth is *chalk*, and the most perfect chalk is that which is called *whiting*, which is therefore useful in many experiments, so that it is worth while to know what air it contains. From seven ounces of this substance, I got, in an earthen retort, 630 ounce measures of air, by which it was reduced to four ounces. Every portion of the air contained about one-third that was not fixed air, the standard of which was 1.36, 1.38. Again, from six ounces of whiting, I got 440 ounce measures of air, about half of which was fixed air, and the remainder of the standard of 1.4. The whiting was reduced to three ounces and 312 grains.

In order to try whether any peculiar kind of air might be procured from whiting saturated with acids, I moistened some, which had been well calcined, with water impregnated with vitriolic acid air; and then by heat expelled from it ninety ounce measures of air, the former part of which was more than three-fourths fixed air, and the residuum of the standard of 1.5. The last portion had less fixed air in it, and the standard of the residuum was 1.44. The substance was rendered
black

black and hard, but in spirit of salt it became white and soft.

When *quick lime* is suffered to fall in the open air, it first attracts moisture, and then that moisture gives place to fixed air. From three ounces and a quarter of this fallen lime I got 375 ounce measures of air, of which about one-fifth was fixed air, and the standard of the residuum was 1.4.

Iron ores may be pretty well distinguished by the quality of the air that they yield by heat, as well as by their weight, and external appearance. The residuum of the air from other stony substances, after the fixed air is separated from it, I have always found to be phlogisticated, but that from iron ore is inflammable.

Three ounces and one-half of *white spatulose iron ore* yielded 560 ounce measures of air, of which at the first one-third was fixed air; then only something more than one half, and again at the last a third. The standard of the residuum was about 1.7, and inflammable. The substance was reduced to one hard mass, and the bottom of the earthen retort was melted along with it.

I tried one iron ore that was of a *light* colour, and another of a *darker*. Six ounces of the lighter coloured ore yielded 750 ounce measures of air, of which at the first two-thirds were fixed air, then

only a little more than a half, and at the last a fifth. The residuum of the middle portions of this air only was inflammable, that of the rest phlogisticated; the standard of it about 1.7. It was reduced to a hard black slag full of cavities.

Four ounces of the dark coloured ore yielded 510 ounce measures of air, the quality of which varied very much, like that in the preceding experiment, and the air was not more strongly inflammable.

There is, I believe, some iron in the substance that is called *black lead* (*molybdena*) and therefore I mention the experiment that I made with it in this place. From half an ounce of it I got twenty-five ounce measures of air, of the standard of 1.6, and 1.42. I have no note of any part of its being fixed, or inflammable. It had lost only eighteen grains in weight. From eight ounces and a half of another kind of black lead, I got sixteen ounce measures of air, one fifteenth, or one twentieth of which was fixed air, and the rest inflammable, burning with a blue flame.

I did not pursue these experiments on ores to any great extent; but having some *stream tin*, I found that 110 grains of it, gave twenty ounce measures of air, a small portion of which was fixed air, and the remainder of the standard of 1.44, and at last 1.34.

From

From two ounces and one fifth of *steatites*, I got forty three ounce measures of air, which had the slightest appearance of containing fixed air. The remainder was thoroughly phlogisticated, except that, at the last, it was of the standard of 1.65. It came out of the retort a yellow mass, but powdery, as it was put into it.

Two ounces of *terra ponderosa*, gave twenty-six ounce measures of air, without any mixture of fixed air, the standard of it 1.62, 1.42, and 1.29. The substance was concreted into one mass, but was easily broken by shaking the retort, and then it did not appear to be changed in its external appearance.

Two ounces of *black wad* from Derbyshire, yielded eighty ounce measures of air, no part of which was fixed air, but all better than common air, the standard of it being 1.05. This circumstance may help to account for this substance taking fire, and burning as it does, when it is mixed with linseed oil. For if by any means it is so far heated, as to give out its pure air, this must assist the combustion; and the chemical attraction between the phlogiston in the oil, and the dephlogisticated matter in the wad may, without its assuming the form of air, be the cause of the mass becoming hot.

Seven ounces of *fluor* gave eight ounces of air, a small proportion of which was fixed air, and the rest

rest of the standard of 1.45. It was melted into a hard mass. Six ounces of white fluor yielded in all ten ounce measures of air, of which the lightest portion imaginable was fixed air, the rest of the standard of 1.34, and 1.3. In this experiment the bottom of the retort was quite dissolved. N.B. There was no appearance of fluor acid in the water in which this air was received, and the melted mass gave fluor acid air with oil of vitriol.

From four ounces of a kind of *sand-stone*, I got seventy five ounce measures of air, a small portion of which was fixed air, the standard of the rest, for the most part, 1.75, and at the last 1.35. When taken out of the retort, it weighed three ounces and three fourths. That part of it which was next to the bottom of the retort was whiter than the rest, but very hard, adhering to it; and, what was pretty remarkable, the remainder had acquired just as firm a texture as it had before it was pounded for the purpose of the experiment.

Five ounces of a fine *white sand-stone*, yielded about ten ounce measures of air, containing a little fixed air, and the rest of the standard of 1.6. This also was again reduced to a stone quite as compact as it had been before it was pounded.

Six ounces of another *sand-stone* yielded 102 ounce measures of air, of which a very small portion was fixed air, and the rest of the standard of

1.57, and 1.35. This also was reduced to a hard dark coloured stone, having separated itself from the retort about a quarter of an inch, except at the bottom where it adhered to it.

From one ounce and 175 grains of *belemnite*, I got 320 ounce measures of air, of which at the first one sixteenth was fixed air, the rest of the standard of 1.75, 1.55. All the air came while the heat was very moderate.

From four ounces of *crystals of quartz*, I got 25 ounce measures of air, a very small portion of which was fixed air, the rest being of the standard of 1.8, and 1.44.

From seven ounces of a *granulated quartz*, I got about ten ounce measures of air, containing a little fixed air, and the rest of the standard of 1.42. It came out of the retort a loose friable substance, weighing six ounces 290 grains. The retort was cracked, or more air would probably have been procured.

From one ounce and eighty four grains of *mica*, I got twelve ounce measures of air, of which no part was fixed air, but of the standard of 1.4, and 1.35.

From 120 grains of *talc*, I got a quantity of air, but the retort being cracked at the beginning of the process, I took no account of the quantity. Part of it was evidently fixed air, and the rest of the standard

dard of 1.4, and a candle burned in it. The substance was reduced to a dark hard cinder, adhering to the retort.

From four ounces 355 grains of *crystalized glass*, in the form of a whitish stone, I got twelve ounce measures of air, which contained no fixed air, and of the standard of 1.42, 1.36, and 1.31. Perhaps I used a greater degree of heat than the glass had been subjected to before. Otherwise this experiment might help to account for lava giving some quantity of air, though it had been in a state of fusion, having afterwards crystalized, like this glass.

The last experiment that I shall mention was made with *pit coal*. Three ounces of such coal as we have at Birmingham, gave 700 ounce measures of air, of which I could not be sure that any portion was fixed air. It was all inflammable, the first portion of it burning with a white lambent flame, and the last with a blue one.

To the substances from which I had endeavoured, at different times, to extract air by heat, it may be just worth while to mention *crude antimony*. From one ounce of it, in a glass vessel, and with a red sand heat, I got very little air, not more than its bulk. The last portion was in a great measure fixed air, and the residuum extinguished a candle. The antimony on which this experiment was made, and which had been pounded, formed a concrete mass

mass when taken from the fire, being mixed with any of the acids.

A degree of heat sufficient to bake *clay*, evidently expels fixed air from it. In order to ascertain this fact, I filled a gun-barrel with tobacco-pipe clay, and, putting it into the fire, I received the air that came from it, in several portions; but the whole was not more than about five times the bulk of the clay. The first produce was inflammable; but afterwards the air was fixed, precipitating lime in lime-water, and being readily absorbed by water. I never met with purer fixed air.

No *calx* of any metal on which I made the experiment yielded inflammable air, but all of them fixed air, and generally in great plenty. *Rust of iron* gave a great deal of air, two thirds of which was fixed air, and the rest was not affected by nitrous air, and extinguished a candle; so that the whole produce seemed to be fixed air, only with a larger residuum than usual of that part which is not miscible with water. At another time, however, I got from the rust of iron fixed air that was very pure, there being little of it that was not miscible with water.

N. B. That part of the rust on which the focus of the lens fell, turned very black.

I observed that both the *grey calx of lead*, and *litbarge*, yielded fixed air, and that a great quantity

quantity of fixed air is contained in red lead, and in other preparations of that metal.

I got a little air by means of the burning lens in quicksilver, from *cinnabar prepared with antimony*; but not enough to form a judgment of the quality of it. From common *vermillion* I got more air, viz. about forty times its own bulk, and it was all fixed air, being readily absorbed by water. This substance, like the rust of iron, turned black in the focus of the lens.

SECTION II.

Air from saline Substances.

MOST saline substances, I believe, contain more or less fixed air; and it may be worth while to examine what *quantity* of it may be extracted from each of them, and also the quality of the residuum, which I find to differ considerably in different cases. But this may depend, in a great measure, upon the state of the water in which the experiments are made. A few observations that I

have had occasion to make of this kind may be just worth noticing.

Both *vitriolated tartar*, and *Glauber salt*, which I have often occasion to make in the course of my experiments, I find contain fixed air. Dissolving a quantity of vitriolated tartar, which was formed in making spirit of nitre, and collecting the air that came from it, I found one twelfth of it to be fixed air and with an equal quantity of nitrous air, the measures of the test for the remainder were 1.3. At another time I filled the retort in which the salt was contained with boiled pump water, and then I found no fixed air in it; having, I suppose, been absorbed by the water, and the measures of the test for the remainder were 1.46. Again I dissolved a quantity of this salt in pump water, and then found one fourth of the whole to be fixed air; the pump water itself containing a good deal, and the measures for the residuum were 1.44.

From half an ounce of *vitriolated tartar*, in a gun-barrel, I got about an ounce-measure, and a half of air, which was chiefly fixed air. The last produce diminished common air a little; but this I attribute to the gun-barrel not having been perfectly cleaned from the materials used in a former experiment.

I also dissolved a quantity of *Glauber salt*, which remained from the process for making spirit of salt,
and

and I found the residuum of the fixed air to be sensibly worse than common air.

The first experiment that I made upon *alum*, was with the sun-beams, in quicksilver; when I got from it a little air, which appeared to be fixed air, by extinguishing a candle, and by being readily absorbed by water. I repeated the experiment with the same result. The quantity of air extracted from a piece of alum, was about one third of its bulk; but I imagined that a little, though not much, more might have been extracted, by a longer continuance of the operation.

I observed, upon this occasion, that I could calcine only a given quantity of alum in a given quantity of air; and that when this was saturated, I could only keep the alum in a fluid state by heat. But it was easily calcined in vacuo; and as the receivers in which the calcination was made became very moist, it is pretty evident that this operation is performed by the mere expulsion of the water which enters into the composition of this salt; so that when the surrounding air can take no more water, that calcination can proceed no farther. I also observed, upon this occasion, that when I had calcined a quantity of alum in a given quantity of common air, the air was not diminished, or in the smallest degree injured, by the operation.

After this, I endeavoured to get air from *calcined alum*, with a burning lens; and I did get a little: but I made no other observation upon it, than that it was not diminished by nitrous air. But when I put a quantity of calcined alum into a gun-barrel, I got from it a considerable quantity of air, part of which was fixed air, precipitating lime in lime-water, and the remainder did not differ from the residuum of fixed air, extinguishing a candle, and neither affecting common air, nor being affected by nitrous air.

N. B. The pure air from the alum, and the inflammable from the iron of the gun-barrel, would produce the fixed air.

In dissolving alum, in order to get some earth of alum, I observed that air was discharged from it. This I collected, and found it to contain very little fixed air, and the measures of the test for the residuum were 1.12. At another time I had the same result, but the air was not quite so good, though purer than common air.

Precipitating a solution of alum with pot ash, I caught the fixed air, which was discharged in great abundance; and examining the residuum, found it to be better than common air, in the proportion of 1.2 to 1.3; the diminution being in that proportion when mixed with equal quantities of nitrous air.

From

From one ounce of *calcined alum*, very white and clean, I got sixty ounce measures of air, without any fixed air, or the least imaginable, and so pure, that with two equal measures of nitrous air, the test was 1.4. Still the residuum had an acid taste, so that with more heat, it is probable that more, and purer air, would have been produced.

The *metallic salts*, if they gave any air at all, gave fixed air, which I find to be contained in most saline substances. I shall recite a few experiments of this kind, without any particular regard to the order of them.

I could get no air whatever from *sugar of lead*, or from *nitre of lead*. The former melted into a liquid substance; the latter changed from white to a dull grey colour, and broke into powder, with a crackling noise.

All the kinds of *copperas* gave fixed air. I first tried common *green copperas* in quicksilver. It dissolved into a great quantity of water, but the air produced from it was not one twentieth of its bulk. Half of this air was readily absorbed by water, and the remainder was too small to be examined. I repeated the experiment on calcined copperas, both in a gun-barrel, and likewise in a tall glass vessel filled with sand; but the produce, in all the cases, was fixed air. Half an ounce of calcined copperas yielded near a pint of air.

When I had extracted air from the calx of green copperas in a glass-vessel, I put the same materials into a gun-barrel; but still I extracted nothing from them besides fixed air, mixed with acid air, as appeared by the extremely small bubbles to which the large ones were presently reduced in passing through water.

When I made the experiment on *blue vitriol*, which consists of oil of vitriol and copper, in quicksilver, the result was the same as with the green copperas, except that much less water was produced.

White vitriol, which consists of oil of vitriol and zinc, gave ten times as much air as the other kinds. Half of it was absorbed by water, and a candle burned in the remainder.

Mercurial nitre gave a great quantity of air in quicksilver, and this was pure nitrous air; but possibly the nitrous acid being let loose from this substance, had produced the nitrous air by dissolving the quicksilver.

White lead yielded air in great plenty, by the heat of the burning lens, and it was all pure fixed air.

From four ounces of *white lead* I expelled, in an earthen retort, 240 ounce measures of air, before the retort was dissolved by it. Of the first produce there remained one third, not fixed air, of the standard of 1.36; and towards the last, the residuum was of the standard of 1.28, when with the common air it was 1.23.

S E C.

SECTION III.

Air from Substances of a vegetable Origin.

TARTAR is a substance concerning which there has been a great diversity of opinions among chemists. On this account some of my chemical friends requested that I would examine what kind of air it yielded in different circumstances. Accordingly, to satisfy them, and my own curiosity at the same time, and without any particular expectation (for I had formed no opinion whatever with respect to it) I began with putting a small quantity of the cream of tartar into some oil of vitriol, contained in a phial with a ground stopper and tube (which is the method that I usually employ to procure vitriolic acid air) and, with the flame of a candle, I made it boil.

The acid presently became black, and the mixture yielded a great quantity of air, till it was quite viscid; when, there being some danger of choaking the tube, I withdrew it. The air was at first half fixed air, making lime water turbid, and half inflammable, burning with a lambent blue flame; but towards the last two thirds of it was inflammable. I did not use more than a few penny-weights

of the tartar, and the quantity of air exceeded two quarts, and much more might certainly have been procured. The next day the matter, which I had poured out of the phial, had the consistency, colour, and smell of treacle; except that there were some small concretions in it. Some time after I took the residuum above-mentioned, and putting it into a glass vessel, I again extracted from it, in a sand heat, a large quantity of air, as much as before, and exactly of the same kind. In the middle of the process, when the production of air was most copious, it was very turbid; and when any of the bubbles burst in the open air, they were perceived to have a strong smell of treacle.

After this I ceased to make use of oil of vitriol, in order to try what air the tartar would yield of itself; and I presently found that the acid had contributed nothing at all to the air that I had got from it. From an ounce of cream of tartar, in a glass vessel, and a sand heat, I got 170 ounce measures of air, the first portions of which were almost pure fixed air. The residuum, however, was inflammable, and burned with a blue flame. At last only about two thirds of the air was fixed air, and the rest inflammable. In the greatest part of the process, the air was very turbid; but it was so in the recipient, and the part of the tube next to it, a considerable time before it was turbid in the rest of the tube,

tube, or in the glass vessel that contained the materials. Towards the end of the process the empyreumatic oil came over, which was very offensive, though, at first, the smell of the air had been rather pleasant, resembling that of burnt sugar.

I repeated this experiment, and again got about 170 ounce measures of air from an ounce of cream of tartar, of which thirty eight ounce measures were inflammable, and the rest fixed. It burned with a large white flame, but at last with a light blue one, owing, I suppose, to the mixture of fixed air in it.

That cream of tartar should yield *fixed air* will not be thought extraordinary; but its yielding inflammable air, seems to shew that it had acquired a good deal of the consistence of vegetable matter, or of pit-coal, since those substances yield the same kind of air.

After this, neglecting the produce of air, I simply calcined a quantity of cream of tartar, in a red heat, in a glass vessel filled up with sand; and observed that it lost about half its weight. Notwithstanding its calcination in a red heat, this substance obstinately retained a great deal of its fixed air, in which it resembles chalk. For when I put this calcined cream of tartar into spirit of salt it yielded a considerable quantity of air, which I found to be fixed air, with a phlogisticated residuum. It also, effervesced in the same manner, and no doubt gave the

the same kind of air in oil of vitriol, and spirit of nitre. But even spirit of salt did not dissolve the whole of it.

To observe the phenomena of this calcination more particularly, I made the process in an open crucible, which I kept in a red heat a long time. But when there was no appearance of any farther change, and the substance was pretty hard, I took it from the fire, on which it presently assumed a blackish, or dirty brown colour. Spirit of salt dissolved this substance with as much rapidity, to all appearance, as it had done the mere black coal of tartar in the former experiment, and expelled as much air from it. It still, however, did not dissolve the whole: for a dirty powder remained undissolved.

I threw the focus of the lens upon a piece of fine *white sugar*, in quicksilver. It was readily melted and converted into a brown substance, yielding about two thirds of its bulk of air, one third of which was readily absorbed by water, and the remainder extinguished a candle. I repeated the experiment with a brownish powdered sugar, with the same result, excepting that more air was generated from this than from the white sugar, in proportion to their bulks.

From two ounces and three quarters of *wood ashes* I got, in a very strong heat, 430 ounce measures

tures of air, of the first portion of which one tenth, of the second one third, and of the third one half was fixed air. The residuum of the second portion was of the standard of 1.6, and that of the third 1.7. It extinguished a candle; so that the air came properly from the *ashes*, and not from any remaining particles of the charcoal mixed with them. After the process, the ashes weighed 839 grains. Being exposed to the open air one day, they weighed 842 grains, and, perhaps with more heat than before, yielded fifty ounce measures of air, of which about an eighth was fixed air, and the standard of the residuum was 1.38, and 1.41. A candle burned in it; so that it is evident some of the deplogificated part of the atmosphere had been imbibed by these ashes. They then weighed 789 grains and a half.

From three ounces of *pit-coal ashes*, I got air, the standard of which was 1.7, and extinguished a candle. I took no note of the quantity of fixed air, and through an accident in the process most of the air escaped.

It is well known that all fermented liquors, that are not quite flat or vapid, contain fixed air; and I had the curiosity to try, what proportion of this air is contained in different kinds of wine, and in wines in different states. For this purpose, I took one of the phials with a ground-stopper and
tube,

tube, containing an ounce-measure and a half, and filling it accurately with each species of wine, I plunged it into a vessel of water, which was set on the fire to boil, receiving the air in quicksilver. The air that I got from all kinds of fermented liquors was pure fixed air; but, except champaigne and cyder, it was in much less quantity than I expected; the results being as follows.

The quantity of air contained in

Madeira, was	-	$\frac{1}{100}$	of an ounce-measure.
Port of six years old		$\frac{1}{48}$	-----
Hock of five years old		$\frac{1}{14}$	-----
Barrelled Claret	-	$\frac{1}{12}$	-----
Tokay of sixteen years		$\frac{1}{20}$	-----
Champaigne of two years	2		-----
Bottled Cyder of 12 years	$3\frac{1}{4}$		-----

Some champaigne sparkles much in consequence of containing much air; but there is a kind of champaigne which does not sparkle, and contains very little air. The difference, as I was informed, when I made enquiry concerning it, in that part of France where the wine is made, is owing to this; that when they wish to have the wine sparkle, they check the fermentation as much as possible at the time that the wine is made; so that the fermentation going on gradually, the fixed air produced

duced by it is absorbed by the liquor: whereas, when they do not chuse to have it sparkle, they let it ferment freely, like any other kind of wine.

In other cases, therefore, where fermented liquors contain much air, as in most kinds of malt-liquor, cyder, and our English made-wines, I take it for granted, that the fermentation is either purposely checked, or that the liquor is of such a nature, that the fermentation will necessarily continue a long time, after it is put into the cask or bottle.

I once found that a quantity of port-wine contained its own bulk of fixed air; but I now imagine that the wine was not genuine, but must have been made chiefly of cyder. Perhaps this may not be a bad method of distinguishing genuine foreign wines from compositions made of cyder.

SECTION IV.

Air from Animal Substances.

I Had observed, that animal substances, in putrefying, discharge air that is in part fixed, and part inflammable. Being willing to find the *proportion* of each of these kinds of air, in the different stages of the putrefactive process, as well as the *whole produce* of both kinds, I took a piece of the lean muscular part of mutton, weighing 102 grains, on the 13th of September, 1776, and put it into a jar filled with quicksilver, standing inverted in a basin of the same, and placed it near the fire, where the heat was variable, but at a medium of about 100 degrees of Fahrenheit.

On the 15th I took from the mutton half an ounce measure of air, two thirds of which was fixed air, making lime-water turbid, and the rest was strongly inflammable. On the 16th it had yielded one third of an ounce measure of air, of which the fixed air and the inflammable were exactly in the same proportion to one another as before; but the inflammable air at this time was all fired at one explosion, and without that redness
in

in the flame that I had perceived before. On the 19th I took from it about half an ounce measure of air, three fourths of which was fixed air, and the rest inflammable.

After this I removed the mutton and quicksilver into the common temperature of the atmosphere, where they continued to the 13th of January following, in all which time very little was added to the air that had come from it before its removal. I then, however, took from it half an ounce measure of air, and it was all pure fixed air, without the mixture of any thing inflammable in it. Then placing it near the fire as before, it presently yielded another half ounce measure of air, which was also wholly fixed air.

Observing that it stood near twenty-four hours after this without producing any more air, though it was in the same degree of heat, I plunged the whole into a pan of water, and made it boil; by which means I got from it about one eighth of an ounce measure of air, the whole of which was fixed air; at least the residuum was not larger than is usual in pure fixed air; for it was too small a quantity to make an experiment upon with the flame of a candle. After this I kept it in a boiling heat a considerable time, without getting from it any air at all. It appears therefore, that this piece of mutton yielded in all $2\frac{1}{4}$ measures of air,
of

of which $2\frac{1}{2}$ was fixed, and the rest inflammable, and that all the inflammable part was exhausted a considerable time before the fixed air.

On the 13th of March, 1780, I took two dead mice, of about equal size, and put them into two separate cups, under different jars of common air, of very nearly equal capacities, one of them containing 155 ounces of water, standing in quicksilver, and the other 160 ounces, standing in water.

Leaving them in the country to the care of a person who supplied the vessels in which they stood occasionally with water or quicksilver, I went to London, and after my return, in the beginning of August, I found, by marking the vessels, and measuring them afterwards, that the air in the vessel which had stood in water was reduced to 140 ounce measures; and on the 28th of August it was reduced to 135, but after standing a fortnight longer, it was not sensibly diminished any farther. The air in the vessel which had stood in quicksilver was not sensibly diminished at all.

Admitting lime water to this vessel, it presently became turbid; but this being a slow diminution I removed the vessel after some days to a trough of water, and then found that the air contained in it made lime water exceedingly turbid; and agitating this air in small portions it was presently reduced to 125 ounce measures; so that all the quantity
diminished

diminished seems to have been fixed air, making lime water turbid, and being absorbed by water in the very same manner.

The air in the vessel which had stood in water, notwithstanding the opportunity there was for the fixed air deposited by it being readily absorbed, made lime water very turbid; and by agitation in small portions this air was reduced to 130 ounce measures. Upon the whole it appears, that the diminution in both of these cases was nearly equal, viz. a little more than one fifth.

In these experiments the two mice were thoroughly putrefied, and indeed quite dissolved, and no doubt had yielded all the air they were capable of yielding. But if the experiments on the putrefaction of mice in quicksilver recited above be compared with these, it will be found that the addition of fixed air, or air of any other kind, from the putrefied mice was quite inconsiderable, viz. an ounce measure and half of fixed air, and half an ounce measure of inflammable from each.

It is true that mice putrefying in water yield perhaps more fixed air than in this proportion; but here they putrefied in air only. And that a very inconsiderable quantity is produced in these circumstances, is evident from there being little or no increase of the air when it is confined by quicksilver, which could not imbibe fixed air, if

any had been discharged from the putrefying mice. It will be found hereafter, that water is a necessary ingredient in the constitution of both fixed and inflammable air.

It might be questioned, whether the fixed air contained in our aliments, can be conveyed by the course of circulation into the blood, and by that means impregnate the urine. I have found, however, that it may do it; having more than once expelled from a quantity of fresh-made urine, by means of heat, about one fifth of its bulk of pure fixed air, as appeared by its precipitating lime in lime water, and being almost wholly absorbed by water; and yet a very good air-pump did not discover that it contained any air at all.

It must be observed, however, that it required several hours to expel this air by heat; and after the process, there was a considerable whitish sediment at the bottom of the vessel. This was, probably, some calcareous matter with which the fixed air had been united; and by this fixed air, the calcareous matter, which would otherwise have formed a stone or gravel, may have been held in solution; and therefore, drinking water impregnated with fixed air, may, by impregnating the urine, enable it to dissolve calcareous matters better than it would otherwise have done, and may therefore be a means of preventing or dissolving the

the stone in the bladder, agreeable to the proposal of my friend Dr. Percival.

From four ounces of dry *ox blood* I got 1200 ounce measures of air, and I conjectured that not less than 200 ounce measures escaped. It contained no fixed air. The first portion of it burned with a large lambent white flame, the middle portion fainter, and the last was hardly inflammable at all, but had a slight blue flame. What remained of the blood weighed 255 grains, and was a very good conductor of electricity, which is not usually the case with the charcoal of animal substances.

H₂

PART

P A R T III.

VARIOUS PROPERTIES OF FIXED AIR.

SECTION I.

The Effects of fixed Air on Animals and Vegetables.

INSECTS and animals which breathe very little are stifled in fixed air, but are not soon quite killed in it. Butterflies, and flies of other kinds, will generally become torpid, and seemingly dead, after being held a few minutes over fermenting liquor; but they revive again after being brought into the fresh air. But there are very great varieties with respect to the time in which different kinds of flies will either become torpid in the fixed air, or die in it. A large strong frog was much swelled, and seemed to be nearly dead, after being held about six minutes over the fermenting liquor; but it recovered upon being brought into the common air. A snail treated in the same manner died presently.

While

While I was making experiments on the fixed air produced by the fermentation of beer, in a public brewery, which was a considerable time before I attempted to procure it in any other manner, I had the curiosity, among other things, to try what effect it would have on the *vegetation of plants*, and the *colours* of some delicate flowers; both which I could easily suspend within the region of fixed air over the fermenting vats. The result of a few experiments, which I made in these circumstances, was as follows.

Fixed air is presently fatal to vegetable life. At least sprigs of mint growing in water, and placed over the fermenting liquor, will often become quite dead in one day; nor do they recover when they are afterwards brought into the common air. I am told, however, that some other plants are much more hardy in this respect.

A red rose, fresh gathered, lost its redness, and became of a purple colour, after being held over the fermenting liquor about twenty four hours; but the tips of each leaf were much more affected than the rest of it. Another red rose turned perfectly white in this situation: but various other flowers, of different colours, were very little affected. These experiments were not then repeated, as I wished they might be done,

in pure fixed air, extracted from chalk by means of oil of vitriol.

After this I found a contrary opinion to prevail, viz. that fixed air is so far from being *destructive* to vegetation, that it is the proper *pabulum* of vegetables; making them to flourish much more than they could do in other circumstances; and that, instead of discharging the colour of rose leaves, it is a means of preserving them, and all other most delicate flowers, in the greatest perfection. I therefore made the following experiments.

On the 5th of June, 1776, I put two sprigs of mint into two equal jars, filled to the same height with pure fixed air, extracted from chalk by oil of vitriol, the lower parts of each jar containing equal quantities of the same rain water; with this difference, that into one of the jars I conveyed a little oil, to prevent the too quick absorption of the fixed air by the water. Also in the same trough of water, in which stood the jar without oil, I placed another jar, filled to the same height with pure fixed air, without any plant. I presently observed that the water rose in all the jars exactly alike, except in that which had the oil on the surface of the water; and the next morning both the plants appeared to be quite dead, their stems and leaves having become almost black
and

and flaccid. After two days, when there was evidently no probability of the plants recovering themselves, I took them out, and found the air to which they had been exposed not in the least changed, being just as much absorbed by water as other fixed air.

Thinking it possible, that though these plants died in a total change of atmosphere, they might, notwithstanding, have borne a *partial charge* of it, I took three other plants, and put one of them into a jar of air of which *two thirds*, another into one of which *one half*, and a third into one of which *one fourth* was fixed air. But, to all appearance, all these plants died as quickly as the two former had done, which I believe was, in fact, almost instantly: for the cessation of vegetable life must have considerably preceded such *visible effects* of it as the blackness and flaccid state of the leaves and stalks.

To close this set of experiments, I, in the last place, put only *one eighth* part of fixed air to two plants which had been growing some time very well in phials of water, over which I had placed jars full of common air only, in order to avoid wetting the plants, or doing them the least imaginable injury, in any respect. But, notwithstanding this, and though very little indeed of the fixed air could be supposed to remain a long time, of so very small a quantity, exposed to so very large a surface of wa-

ter, in a few days the tips of the leaves, even to the tops of the plants, turned black: both of them soon shewed evident marks of decay: one of them died in about ten days, and the other did not survive more than about three weeks. For in such a languishing state, it is not easy to say at what precise time a plant must be pronounced to be properly dead.

In the next place I tried the effect of water impregnated with fixed air on the *roots* of plants.

In one case, a sprig of mint, in the impregnated water, grew better than a similar plant in the same water not impregnated with fixed air; but another plant grew much worse than its companion in common water. Besides, though it should appear that, for a time, a plant should grow better in this kind of water, it may, perhaps, be attributed to the effects of *stimulus* only, which is not peculiar to fixed air, but might result from the action of any other acid. And when I put a little common salt, or even a little spirit of nitre into the water in which the plants were growing, I imagined that, for some time, it rather promoted their growth. Also, though, in general, plants die almost immediately in water impregnated with nitrous air, yet in one case of this kind, when the superfluous nitrous air was carefully let out under water, so that no part of it was decomposed in contact with the water, the plant grew in it remarkably well.

The

The few observations that I have made on the growth of plants in water impregnated with fixed air, but which I do not pretend to be sufficient to decide the question, were the following. On the 20th of August 1776, I gathered two slips of mint, and likewise two small plants of the same kind, with roots; of which I put one of each kind into an eight-ounce phial of rain-water, and the others into other similar phials, filled with the same water impregnated with fixed air; putting at the mouth of each of them a little soft clay, to prevent the too easy escape of fixed air from those that contained it, and to put the others, as nearly as possible, into the same circumstances.

For some time all these plants appeared to flourish equally well; but after a week it was evident that the slip of mint in the impregnated water, grew better than its companion. On the 4th of September the plant in the simple water was in a dying condition, and the other began sensibly to languish, and was dead, I think, about a week after the other.

The two plants with roots grew very well; but that in the simple water much better than the other; and more of the water had been exhaled from the phial, the reverse of which had been the case with the slips. On the 24th of September the plant in the fixed air was absolutely dead; but the other in
simple

simple water was very flourishing on the 28th, when I put an end to the experiment. Examining the phials of impregnated water, I found that neither of them had intirely lost its fixed air. That in which the sprig of mint had grown, still contained one sixth of its bulk of fixed air, and the water in which the plant with its root had grown, which was a much longer time, retained, however, one twelfth of it.

To try the effect of different *stimuli* on the roots of plants, I first put into phials containing an ounce measure and a half of common water, small quantities of common salt, from one grain to twelve, and more. In all those which contained more than twelve grains, the plants died immediately, but in that phial the plant lived a few days; and the rest died, in their order, to that which contained three grains of salt, which seemed to grow as well as the plant in simple water. And it was remarkable that not only this plant, but also those which had died seemed to flourish more *at the first*, than those which grew in simple water: and that which had three grains of salt, and also that which had one grain, continued to live after the plant in simple water was dead in the same room. This was in my laboratory, a place unfavourable, indeed, to any vegetation, but equally so to all.

Sprigs

Sprigs of mint in one and a half ounce phials, containing one, and even two drops of the strongest nitrous acid flourished very well, better, seemingly, than those in mere water; but in water containing more of this acid, they died instantly.

I am far from pretending that these few experiments on the vegetation of plants in water impregnated with fixed air, are decisive; but I think they shew that a very great number of experiments, and those uniform in their result, are necessary to determine this question. When some plants grow better, and some worse, it makes it probable that the difference in the growth depends upon some other circumstance than the water in which they grow.

While I was attending to the comparison of the growth of plants in dephlogisticated and common air, I at the same time made a few farther experiments on the growth of plants with their leaves exposed to fixed air, though I was pretty well satisfied, from the experiments recited above, that this kind of air is undoubtedly injurious to plants growing in it. I wished also, once more, to try the effect of inflammable air, with respect to vegetation.

Accordingly, in the month of April 1777, I introduced a sprig of mint into a phial of air, one third fixed and the rest common; and having only once supplied it with fresh fixed air (when the bulk
of

of the former was absorbed by the water) I observed, that on the 3d of May following, there were black specks on several of its leaves, and in the course of a week it was almost wholly black, and evidently dead. It had not grown at all.

At the same time I put another similar plant into a jar of half fresh made inflammable air and half common air, but it died presently. I found, however, by subsequent trials, that plants would bear a greater proportion of inflammable than they would of fixed air; so that from the circumstance of plants merely *living* in a proportion of fixed air, it cannot be inferred that it is *of itself*, at all favourable to their growth.

The few experiments that I had an opportunity of making before, left me altogether undecided with respect to the effect of water impregnated with fixed air on the *roots* of plants. But the many experiments that I have made since, in 1777, and 1778, have not left a shadow of doubt on my mind, that such water is hurtful, and finally fatal to the plants growing in it, at least to sprigs of mint; for I did not make the trial with any other plants.

On the 28th of May I placed, in a green house, and not in my laboratory, as in the experiments mentioned before, three sprigs of mint, with their roots in phials of water impregnated with fixed air, and three other plants of the same kind
with

with their roots in the same water unimpregnated. After a week I changed the impregnated water, on account of the mouths of the phials being left open, lest the plant should have been injured by putting any thing about them, to prevent the escape of the air from the water.

During two or three days at the first, the plants in the impregnated water were more vigorous than the others; but on the 8th of June following, they all looked much worse than those in the common water. Also those in the common water had long white filaments shooting from their roots, whereas those in the impregnated water had none of them. On the 18th of June, the plants in the impregnated water were all quite dead, their leaves having all fallen off one after another, beginning at the bottom. Examining one of the phials, I found that it contained between one fifth and one sixth of its bulk of fixed air.

I repeated these experiments several times in the course of that summer, generally using many more plants than in these last mentioned, but the result was the same in them all. However, as it generally happened, on what account I cannot tell, that the plants in the unimpregnated water died, though later than the others, I deferred the last and decisive trial till the year following, after which I had no doubt remaining on the subject.

On

On the 4th of May, 1778, I put seven sprigs of mint into pump water impregnated with fixed air, and ten or twelve in the same water unimpregnated, the phials being similar, and I placed them all in a summer house, in the same exposure. I renewed the impregnated water every week, till the 23d of June, when all the plants in the water impregnated with fixed air were dead, the roots being black and rotten; while the other plants were in as flourishing a state as possible, and continued to flourish long after, till I discharged the experiment.

On this occasion I did not observe that the plants in the impregnated water were at any time more flourishing than the others, not even at the beginning; and after a fortnight the difference in appearance, to the disadvantage of those in the impregnated water, was very visible. Those which grew in the common water threw out many white filaments from their roots, many of them so long as quite to fill the phial, twisting themselves in all directions, and exhibiting a very beautiful appearance; whereas there was nothing of this kind in any of the phials of impregnated water. On the contrary, the roots became presently black, and at length rotted quite away.

One of these I had overlooked, and had neglected to change the water; and this plant threw
out

out a few white filaments; but, on renewing the impregnated water, they presently became black and perished.

It was remarkable also, that two of the plants in the impregnated water threw out thick knots of those white filaments in the necks of the phials, just above the surface of the water, but not one of them within the water itself, or ever entered the water. Also, when I took one of these plants, the roots of which were quite perished, out of the impregnated water, and put it into a phial of common water, it threw out new white roots above the place that was decayed, and afterwards grew very well.

Mr. Hey, of Leeds, passing through Calne, where I then resided, happened to see these plants in the last stage of the process, and thought that no experiment could be more satisfactory.

SECTION II.

Of the Change made in fixed Air by the electric Sparks

I Observed in a very early period of my experiments, that by taking the electric spark in fixed air, a part of it is converted into air that is not absorbed by water. I have since repeated this experiment with more care; and though I have never been able to make the whole of any proportion of fixed air immiscible with water by this means, yet I have always so far changed it, that the residuum was more considerable than before, but in different proportions.

I took the electric spark about two hours in a small quantity of fixed air confined in a glass tube by mercury. Before the experiment, one thirtieth of the air was unabsorbed by water, but afterwards one fourth. The glass tube in which this experiment was made became very black in the inside; and as this change is made in mercury by the addition of phlogiston, it looks as if some of the phlogiston, which had made a part of the fixed air, had, by this process, been separated from it; and leaving a greater proportion of dephlogisticated
air

air in the remainder, would necessarily make it less miscible with water. The blackness on the inside of the tubes, in which the electric spark is taken through vitriolic acid air or common air, I before discovered to be mercury supersaturated with phlogiston.

The next time that I repeated this experiment, I attended to the quality of the residuum before and after the process; and the result was such as seems to confirm the above-mentioned conjecture. I took the electric spark an hour and ten minutes in little more than half an ounce measure of fixed air, after which one fifth of the whole was unabsorbed by water, and the standard of the residuum was 0.9. Of the original fixed air about one thirtieth was unabsorbed by water, and the standard of the residuum was 1.0. In this experiment I also observed that the quantity of the air in which I made the experiment was increased about a twentieth part, which I do not pretend to explain.

Again, I took the electric spark an hour in half an ounce measure of fixed air, after which there remained as much residuum unabsorbed by water as had remained in about five times the quantity of the same fixed air in which no spark had been taken. This residuum was also much purer than that of the original fixed air, the standard of it being 0.8, whereas that of the original fixed air had

been, as before, i.e. I repeated the experiment, and found the residuum still greater, but of the same pure quality; and in this case I observed a good deal of the black matter adhering to the inside of the tube.

In the following experiment I observed a farther change in this substance. In a small tube, containing about one fifteenth of an ounce measure of fixed air, I took the electric spark about an hour; after which there was a good deal of the black matter clouding all the inside of the tube; but the lower part of it was covered with something of a yellow colour, like sulphur. In this case the residuum not absorbed by water was between one fourth and one fifth of the whole, and less pure than the former residuums. Had not the dephlogisticated air in the fixed air passed into the mercury, tending to make it a precipitate *per se*? Was not this the cause of the residuum being less pure than before? And does not this experiment also prove, that phlogisticated air may be composed of the same materials with fixed air, viz. dephlogisticated air and phlogiston?

Again, I took the electric spark three hours in a small quantity of fixed air, and observed that it was first increased, and then diminished about one eighth of the whole; the inside of the tube being very black, and below the mercury very yellow,
about

about the space of a quarter of an inch quite round the tube. But that space, or at least part of it, had been above the mercury at the beginning of the process. There remained one third of the air unabsorbed by water, and so impure, that the standard of it was 1.8.

To vary the experiment, I took the electric spark in a quantity of fixed air confined by water, impregnated with fixed air. The quantity was much increased by the air extricated from the water, and after the process by far the greater part of it was incapable of being absorbed by lime water. In the course of this experiment, I observed that water impregnated with fixed air is by no means so good a conductor of electricity as water impregnated with any of the mineral acids.

Again I took the electric spark in fixed air, confined by a little common water, and observed that the blackness mentioned above extended more than a quarter of an inch below the surface of the mercury, in the same manner as the yellow colour had done before. In this case also, the residuum was purer than that of the original fixed air.

Again I took the electric spark half an hour in seven tenths of an ounce measure of fixed air, after which one tenth of it was immiscible with water, and the residuum was evidently better than the natural residuum of the same fixed air. The stan-

dard of that had been 1.0, and of the other about 0.85.

I took the electric spark three hours in about three fourths of an ounce measure of fixed air, after which it was increased in bulk one eighteenth. Water being admitted to it, there remained one sixth unabsorbed. Being examined, the standard was found to be as before, a little better than the residuum of the same fixed air*.

Being desirous of ascertaining whether this change in the constitution of the fixed air was owing to the *light*, or the *heat* produced by the electric spark, or to something peculiar to electricity. I first threw a strong light by means of a burning lens, on some pounded glass, confined in fixed air, for some hours. But though the residuum was by this means a little increased, yet being of the same quality with the common air, I suspected that it was the air which was necessarily introduced through the quicksilver along with the pounded glass. There was no change in the dimensions of the air after the experiment.

I repeated the process with fine glass-house sand, which had been previously exposed to a strong

* The addition of air in these experiments, Mr. Monge found to be *inflammable*, which must have come from the calcination of the mercury, and not, as he supposes, from the decomposition of the *water* diffused through the fixed air. *Mem. De l'Academie des Sciences* for 1786, p. 430.

heat. But though the residuum was increased, the experiment was not, upon the whole, more satisfactory than the former. I also heated bits of crucibles in the same manner, and found the residuum larger than before, in the proportion of 10 to 6.6; but the quality of it was worse. To what this should be owing, I cannot tell.

I once more repeated the experiment with bits of crucibles, and the result was certainly favourable to the hypothesis of a real change being made in the quality of the air by *beat*, but I do not pretend to say that it was decisively so. After the process with fifty six measures of the air there was a residuum of three measures; whereas before the experiment, the same quantity of the fixed air had left a residuum of only two measures. And that the additional measure was not the common air, introduced into the vessel by adhering to the bits of crucibles, was evident from the quality of the residuum, which was the very same, viz. of the standard of 1.1. I also assured myself that there was no fallacy of this kind in the experiment, by introducing the very same bits of crucibles into another equal quantity of fixed air. For I did not find that any sensible quantity of common air had been carried into the vessel along with them.

However, by heating *iron* in fixed air, there can be no doubt but that a sensible quantity of it is converted into phlogisticated air; which agrees

with the experiments that I formerly made by putting pots of iron filings and brimstone into fixed air. The experiments that I made of this kind were the following, in which it will be observed, that, though in some of them, there was an increase of the quantity of air after the process, yet that it was by no means equal to the quantity that remained, unabsorbed by the water; and therefore, there must have been a farther addition made of this kind of air in the process.

After heating turnings of malleable iron in a quantity of fixed air for some time, I examined a part of it, and found that about one tenth of the whole was immiscible with water. Having resumed the process with the remainder, I found a residuum of one fourth of the whole. There seemed to be a small addition to the quantity of air after the first part of the process, but I could not perceive that there was any after the second. I resumed the process a third time, but did not find that I had made more than one fourth of the whole immiscible with water. At another time I heated the same kind of iron in fixed air, till of three ounce measures and three quarters of air there was a residuum of 0.8 of a measure, which was slightly inflammable, burning with a blue flame; and in this case there was no sensible addition to the quantity of air at all. Lastly, I heated iron in three ounce measures of fixed air till there was an addition of 0.4 of a measure to the
quantity

quantity of it; but there was a residuum of one measure and a half not absorbed by water, which burned with a slightly explosive blue flame.

SECTION III.

Miscellaneous Observations on the Properties of fixed Air.

1. *The Acidity of fixed Air.*

FIXED air itself may be said to be of the nature of an acid, though of a weak and peculiar sort. Mr. Bergman of Upsal, who honoured me with a letter upon the subject, calls it *the aerial acid*, and, among other experiments to prove it to be an acid, he says that it changes the blue juice of tournefole into red. This Mr. Hey found to be true, and he moreover discovered that when water tinged blue with the juice of tournefole, and then red with fixed air, has been exposed to the open air, it recovers its blue colour again. Mr. Bewley proved in the most decisive manner, the acidity of fixed air, in the Appendix to the second of my former volumes of Experiments, p. 382.

2. *Fixed Air expelled from Water by boiling.*

The heat of boiling water will expel all the fixed air, if a phial containing the impregnated water be held in it; but it will often require above half an hour to do it completely.

3. *The freezing of Water impregnated with fixed Air.*

Having succeeded in making artificial Pyrmont water, I imagined that it might be possible to give ice the same virtue, especially as cold is known to promote the absorption of fixed air by water; but in this I found myself quite mistaken. I put several pieces of ice into a quantity of fixed air, confined by quicksilver, but no part of the air was absorbed in two days and two nights; but upon bringing it into a place where the ice melted, the air was absorbed as usual.

I then took a quantity of strong artificial Pyrmont water, and putting it into a thin glass phial, I set it in a pot that was filled with snow and salt. This mixture instantly freezing, the water that was contiguous to the sides of the glass, the air was discharged plentifully, so that I caught a considerable quantity, in a bladder tied to the mouth of the phial.

I also

I also took two quantities of the same Pymont water, and placed one of them where it might freeze, keeping the other in a cold place, but where it would not freeze. This retained its acidulous taste, though the phial which contained it was not corked; whereas the other being brought into the same place, where the ice melted very slowly, had at the same time the taste of common water only. That quantity of water which had been frozen by the mixture of snow and salt, was almost as much like snow as ice, such a quantity of air-bubbles were contained in it, by which it was prodigiously increased in bulk.

4. *Fixed Air, how affected by Iron Filings and Sulphur.*

Having observed a remarkable change in nitrous air, by a mixture of iron filings and sulphur, I wished to know whether any alteration would be made in the constitution of fixed air, by the same means. I therefore put a mixture of this kind into a quantity of as pure fixed air as I could make, and confined the whole in quicksilver, lest the water should absorb it before the effects of the mixture could take place. The consequence was, that the fixed air was diminished, and the quicksilver rose in the vessel, till about the fifth part was occupied by it; and, as near as I could judge, the process went
on,

on, in all respects, as if the air in the inside had been common air.

What is most remarkable, in the result of this experiment, is, that the fixed air, into which this mixture had been put, and which had been in part diminished by it, was in part also rendered insoluble in water by this means. I made this experiment four times, with the greatest care, and observed, that in two of them about one sixth, and in the other two about one fourteenth, of the original quantity, was such as could not be absorbed by water, but continued permanently elastic. Lest I should have made any mistake with respect to the purity of the fixed air, the last time that I made the experiment, I set part of the fixed air, which I made use of, in a separate vessel, and found it to be exceedingly pure, so as to be almost wholly absorbed by water; whereas the other part, to which I had put the mixture, was far from being so.

Iron filings and brimstone, I have observed, ferment with great heat in nitrous air, and I have since observed that this process is attended with greater heat in fixed air than in common air.

5. *Iron in fixed Air.*

Though fixed air incorporated with water dissolves iron, fixed air without water has no such power,

power, as I observed before. I imagined that, if it could have dissolved iron, the phlogiston would have united with the air, and have made it immiscible with water; but after being confined in a phial full of nails from the 15th of December to the 4th of October following, neither the iron nor the air appeared to have been affected by their mutual contact.

6. *Fixed Air changed by Incorporation with Water.*

Mr. Cavendish observed that a certain portion of fixed air is no more liable to be absorbed by water than common air. This, he states at about one sixtieth part of the whole. I had the curiosity to try, whether, if I saturated a quantity of water with fixed air, and expelled it again by heat, that very air which had actually been in the water, would not be wholly imbibed by fresh water; and whether I could not, by this means, get a purer kind of fixed air than that which is immediately procured by means of chalk and oil of vitriol. This experiment I made twice, with all the care that I could apply, and found, in both the cases, that even the fixed air which had been in the water, contained as large a portion of that which would not be imbibed by water again, as the air which had been immediately dislodged from chalk by oil of vitriol.

In

In order to be more sure of this fact, I was more especially careful, the second time that I made the experiment, to use every precaution that I could think of, in order to prevent any error in the conclusion. For this purpose, I took rain-water, and boiled it about two hours, in order to get it perfectly free from air; and I began to impregnate it with fixed air a long time before it was cold, and therefore before it could have imbibed any common air; and, in order to expel the air from it, I put it into a phial, which I plunged in a vessel of water set on the fire to boil, taking care that both the phial containing the impregnated water, and the glass-tube through which the air was to be transmitted, were completely filled with the water, and no visible particle of common air lodged on the surface of it. I also received the expelled air in water, which contained very little air of any kind, lest the very small degree of agitation which I made use of, in order to make the water re-imbibe the air, should disengage any air from it. Also, that less agitation, and less time, might be sufficient, I chiefly made use of lime-water for this purpose. But notwithstanding all these precautions, I found a very considerable residuum of air, not less than Mr. Cavendish had stated, that water would not imbibe.

At a time when this residuum of fixed air hardly gave the least sensible whiteness to lime-water, I examined

examined the state of it, and found, by the test of nitrous air, that it was very little worse than common air; two measures of this air, and one of nitrous air, occupying the space of two measures only.

7. *Fixed Air exposed to Heat.*

I exposed fixed air, as well as all the other kinds of air, to a continued heat, and in this case I made use of a green glass tube. I kept it in hot sand a whole day, so hot that one end of the tube was much dilated, but had not burst. Opening it under water, one half of the tube was instantly filled, and the remainder was the purest fixed air. I did not perceive any thing deposited on the glass, as in the case of the marine and vitriolic acid air.

8. *A Source of Deception from fixed Air, contained in Water.*

I observed that, in one produce of air from a solution of bismuth in the nitrous acid, I found a small quantity of *fixed air*, but that when I repeated the experiment, I could not find any appearance of the kind. I afterwards made an observation that will probably explain this diversity of appearances, and which also shews that, unless care be taken

taken that the water in which the experiments are made contain little or no fixed air, mistakes of this kind will certainly be made. For I found at one time that, if any kind of air was made to pass through a quantity of water containing much fixed air, it would attract a portion of it, and would not easily part with it afterwards. At another time, however (I think it was in colder weather) I found that air conveyed through the same kind of water (which was from a pump) did not attract any fixed air. But I have not had leisure to examine the circumstances that might occasion this difference. Of both the *facts* I am very certain.

9. *Of fixed Air in acetous Fermentation.*

As many of my observations related to the *vinous* and *putrefactive* fermentations, I had the curiosity to endeavour to ascertain in what manner the air would be affected by the *acetous* fermentation. For this purpose I inclosed a phial full of small beer in a jar standing in water; and observed that, during the first two or three days, there was an increase of the air in the jar, but from that time it gradually decreased, till at length there appeared to be a diminution of about one tenth of the whole quantity.

During

During this time the whole surface of the liquor was gradually covered with a scum, beautifully corrugated. After this there was an increase of the air till there was more than the original quantity; but this must have been fixed air, not incorporated with the rest of the mass; for, withdrawing the beer, which I found to be four, after it had stood 18 or 20 days under the jar, and passing the air several times through cold water, the original quantity was diminished about one ninth. In the remainder a candle would not burn, and a mouse would have died presently.

The smell of this air was exceedingly pungent, but different from that of the putrid effluvium.

10. *Fixed Air from putrefying animal Substances.*

When I made my experiments on air affected by *putrefaction*, I observed that the water in which the mice were suffered to putrefy, must have transmitted some *volatile effluvium* from the putrefying substances, into the surrounding air. This I supposed must be phlogiston, which putrefying substances certainly do emit, loaded with that matter which affects the nostrils with the sense of smell, concerning which I know nothing. But besides this, I have found that, by this means, water becomes thoroughly impregnated with *fixed air*, discharged, no doubt, from the putrefying substance.

That

That this water might have got *some* fixed air I suspected; but to find that it had got so very much, I own surprized me.

Having put two dead mice into a quantity of water, and examining the process after a month, I found that the water was strongly impregnated with a putrid effluvium, which was very offensive, and that some air, unabsoꝛbed by the water, lodged in the top of the phial; it having been filled with water, and inverted in a basin of the same. With the impregnated water I filled a phial with a ground stopper and tube, and making it boil, I expelled from it about its bulk of air, which, when examined, was found to be all pure fixed air. N. B. The water was very turbid, and during the process it deposited a white matter, resembling a soft mucilage, with some small specks of black in it.

PART IV.

OF THE CONSTITUENT PRINCIPLES OF FIXED AIR.

SECTION I.

Fixed Air contains Water.

THAT water is an essential ingredient in the constitution of fixed air, as well as probably of all kinds of air, is demonstrated by my experiments on *terra penderosa aerata*; and these may serve to explain some of the following more early observations on getting so little air from *chalk*.

Heat, I observed, sometimes is able to expel but very little air from chalk. I kept a very small quantity of chalk in the focus of a burning lens, twelve inches in diameter, and twenty inches focal distance, more than half an hour, when the sun was near its greatest altitude, on the 23d of July; but notwithstanding this long exposure to so intense a degree of heat, it seemed to give as much fixed air when thrown into a vessel of water, acidulated with oil of vitriol, as an equal quantity of chalk which had not been exposed to any heat at all. Of

this, however, I only judged by the visible effervescence, and did not make any attempt to measure the produce of air, in order to ascertain the effect of these different circumstances with accuracy. I have also kept chalk more than a quarter of an hour in the strongest heat of a smith's forge, in a crucible, without making any sensible alteration in it.

When I put a quantity of chalk into a tall glass-vessel, and kept it in as strong a sand-heat as it would bear, without melting, I extracted from it only about its own bulk of air; and this was fixed air.

Terra ponderosa aerata (a substance of which Dr. WITHERING has given us an excellent analysis) gives no fixed air by mere heat. But I find, that when steam is sent over it, in a red heat, in an earthen tube, fixed air is produced with the greatest rapidity, and in the same quantity as when it is dissolved in spirit of salt: and, making the experiment with the greatest care, I find, that fixed air consists of about half its weight of water.

From two ounces of the *terra ponderosa* I got, by means of steam, 190 ounce measures of fixed air, so pure that at first 150 ounce measures of it were reduced by agitation in water to three and a half, and of the last produce, 30 ounce measures were reduced to one. Examining the residuum of the first portion by means of nitrous air, I found it to be of the standard of i.5.

After

After this, attending to the *water* expended in the process, I found that I procured 330 ounce measures of fixed air with the loss of 160 grains of water. According to this, as the air weighed 294 grains, the water in the fixed air must have been 80 parts of 147 of the whole.

In another experiment, having previously found that three ounces of the *terra ponderosa* yielded about 250 ounce measures of fixed air, I attended only to the loss of water in procuring it, and I found it to be about one fifth of an ounce, in two successive trials. The quantity of fixed air would weigh 225 grains, and the water expended about 100 grains; so that, in this experiment also, the fixed air must have contained about one half of its weight of water.

That water enters into the composition of fixed air, and adds considerably to its weight, is farther probable from the solution of *terra ponderosa* in spirit of salt. Because when the solution is evaporated to dryness, and the residuum exposed to a red heat, the weight of the air, and of this residuum, exceeds that of the substance from which it was procured; and it is probable, that a red heat would expel any marine acid adhering to it.

Forty eight grains of *terra ponderosa* dissolved in spirit of salt, and then evaporated to dryness, and

exposed to a red heat, lost four grains, and yielded eight ounce measures of fixed air, which would weigh 7.2 grains; consequently, three sevenths of the weight of the air was something that had been gained in the process, and therefore probably water.

The near coincidence of the results of these different experiments is remarkable, and makes it almost certain, that no marine acid is retained in the terra ponderosa that has been dissolved in it, after exposure to a red heat; that the generation of the fixed air carries off part of the water in the menstruum, and that this part of the weight is about one half of the whole.

SEC.

S E C T I O N I I .

Fixed Air may be procured by Means of nitrous Acid.

THAT *nitrous acid*, and *fixed air*, consist of the same elements, differently combined, will be demonstrated by my experiments on the subject of nitrous acid, and this may throw some light on the following more early experiments.

When heat can expel no more fixed air from charcoal, it should seem that spirit of nitre (if this acid itself be not converted into fixed air) can extract more from it. For when I dissolved, in spirit of nitre, some pieces of charcoal, which had been made with the strongest heat of a smith's fire, long continued, so that no more air could be expelled from them by that means; part of it was evidently fixed air, as appeared by its precipitating lime in lime-water.

One of the most decisive experiments of this kind, was made with spirit of wine, which nobody, I believe, suspects to contain any fixed air. For though it makes lime-water turbid, Dr. Black has justly observed, that this is produced by its union with the *water*, in consequence of which the lime

is precipitated in a caustic state. A doubt, however, might be made, whether the turbid appearance made by the air which I produced from the spirit of wine, was really the effect of fixed air. I endeavoured, therefore, and with success, to pursue the experiment farther, and I did it in the following manner :

From a mixture of spirit of wine and spirit of nitre, diluted with water, I produced a very considerable quantity of air, the greatest part of which, being received in a large body of lime-water, was readily absorbed, making the water very turbid. Waiting about a quarter of an hour, till the precipitated matter subsided, I poured the water from it, and putting a very small quantity of the precipitate into some water out of which the air had been well boiled, I poured a little diluted oil of vitriol upon it, in a phial with a ground stopper and tube, and found it to yield air in great plenty ; and this air, being admitted to lime-water, appeared to be, in all respects, genuine and pure fixed air.

By this experiment it appears, that the substance formed by the union of this air and the lime was really chalk, or lime-stone, yielding genuine fixed air with acids, exactly as other calcareous substances do. The air was first generated from spirit of nitre, and some other principle contained in the spirit of wine : it was then incorporated with lime, .and

and after that dislodged from the lime by the vitriolic acid, and made to appear in the form of air again.

Fixed air was also generated in the solution of iron in spirit of nitre.

Having dissolved a quantity of iron in spirit of nitre, diluted with an equal quantity of water, before the effervescence was over I removed the vessel in which it was contained (which was a tall glass phial) into a sand-heat, and received the air, which was transmitted through a glass tube, luted with a mixture of sand and clay, in phials containing rain water. The quantity of air produced in these circumstances was very considerable, and part of it was unquestionably fixed air, and what is remarkably to the purpose of the experiment, the proportion of fixed air kept increasing as the process advanced, till at the last it was more than one third of the whole. All the rest of the air was nitrous, and after the process some of the iron was found undissolved.

SECTION III.

Fixed Air may be formed by Means of something imbibed from the Atmosphere.

IT is evident, that the solutions of some of the metals in the nitrous acid, which do not immediately yield any fixed air, will do so after they have been exposed to the common atmosphere. This appears in the following experiment:

Upon 108 grains of quicksilver I poured the same weight of strong spirit of nitre, hanging balanced in a pair of scales; when I observed that the mixture lost weight by the escape of air, till it was reduced to 123 grains. After this it gained weight, till it was considerably more than the original quantity, but how much this additional weight was I neglected to take any account of. The mixture was made on the 25th of September, and when it had stood in an open and shallow vessel till the 12th of January following, I distilled the whole of it to dryness, in a glass phial; when I found that one seventh of the air produced from it was fixed air, and the rest dephlogisticated. I took in all, seven ounce-measures, but lost a good deal

deal that escaped by the luting, and by the vessel breaking before the process was over.

I then put the same quantity of quicksilver and spirit of nitre into a clean phial, and distilling it to dryness *immediately*, without giving it any opportunity of communicating with the external air (but not beginning the distillation till the solution was completed) I received in all thirty two ounce measures of air, of which the first fourteen were pure nitrous air, and the remainder pure dephlogisticated air, without the least mixture of fixed air in either of them.

Wood-ashes have also the property of imbibing one of the elements of fixed air from the common atmosphere, but they require considerable time to do this, in any very sensible degree; for when they had been well burned, I have not found that they yielded any air that I could collect after being exposed to the open air a day or two; but that they do become saturated with fixed air in a course of time, is evident from the following experiment.

From about three quarters of an ounce measure of wood-ashes, from which I had, about three months before, expelled as much air as I possibly could, by the greatest heat of a common fire, urged with a pair of bellows, in a gun-barrel of about half an inch diameter, I got by the same process
fifteen

fifteen ounce measures of air, eleven of which were completely absorbed by water, and the remainder burned with a lambent blue flame. The phlogiston requisite for this appearance might come either from the gun-barrel, or from some imperceptible bits of charcoal contained in the ashes.

From twice the quantity of wood-ashes, which had been burned about the same time with the others, in a much wider gun-barrel, I got about twice the quantity of air, the greatest part of which, as in the former experiment, was fixed air, and the remainder burned with a lambent blue flame.

Having taken this air in several portions, I observed that the first contained a much greater proportion of fixed air than the last, though what there was of it seemed to be equally inflammable.

A more decisive experiment relating to the *generation of fixed air* than that which is mentioned above with *wood-ashes*, is one that I made with the *ashes of pit-coal*. Pit-coal itself, distilled in a glass vessel, yields no fixed air, but only inflammable air, which, being fired in a wide-mouthed jar, burns with a bright lambent flame, without explosion. But the ashes of the same pit-coal yielded much air, of which one half was fixed, and the rest inflammable. When I had expelled all the air that I could from a quantity of these ashes, I mixed spirit of nitre with them, and they immediately yielded

yielded as much air as before; and of this one half was fixed, and the rest nitrous. Mixing more spirit of nitre with the same ashes again, the produce was the same as before.

To be more fully satisfied with respect to the above-mentioned experiment with wood-ashes, and also the quantity of fixed air imbibed by them in a given time, I kept the same ashes, and extracted air from them at certain intervals. I also did the same thing with several other substances of a similar nature, and the results were as follows.

On the 18th of April, 1778, I extracted all the air I could from half an ounce of wood ashes, and got about eighty ounce measures, half fixed air, and half inflammable throughout; and on the 25th of the same month I repeated the process on the same ashes, in a gun-barrel, and got from them twenty ounce measures of air, the greatest part of which was fixed air, and the rest inflammable. The ashes were become almost black after the experiment.

June the 2d, I extracted, by heat, in a gun-barrel, from wood ashes from which air had often been extracted before, in the same manner, and the last time on the 9th of May preceding, all the air that they would yield. It was twenty one ounce measures; the first portions of which were half fixed air, and afterwards one third; the remainder

mainder in both cases being inflammable, probably from the iron. A good deal of moisture distilled from these ashes, though they seemed to be perfectly dry. After the process, they weighed eighteen penny-weights, and, judging from their colour, not much more than two thirds of them had been affected by the heat.

On the 23d of October following, the same wood ashes weighed nineteen penny-weights twelve grains, and I got from them, in a gun-barrel, about thirty ounce measures of air, of which more than twenty five ounce measures was pure fixed air, the remainder inflammable, burning with a blue flame. They had not all been equally affected by the heat. After the process, they weighed eighteen penny-weights six grains. That they had attracted fixed air is evident, especially from the last process, in which the greatest part of it was very pure.

On the 18th of April, 1778, I got, from an ounce of *pit-coal ashes*, in a gun-barrel, nineteen ounce measures of air, of which at first two thirds, and at the last one third was fixed air, and the rest inflammable. On the 24th of the same month, I extracted from the same pit-coal ashes (which, as well as the wood ashes in the preceding experiment, had been exposed to the open air in a dish, so as to lay about half an inch thick) 110 ounce measures

measures of air; but with more heat than before. Of the first part of this air one third was fixed air, but of the last hardly any, the remainder being inflammable, burning with a blue flame; but so faintly, that probably the greatest part of it was phlogisticated air.

Heating the same ashes over again, in a shallow iron vessel, and letting them cool, I got from them, by the same process, fifteen ounce measures of air, one third of which was fixed air, and the rest inflammable.

Common pit-coal, I have observed, yields no fixed air, though the *ashes* do; but I have found that one species of pit-coal, called *Bovey coal*, yields fixed air in the first instance, which seems to indicate that there is something of a vegetable nature in that coal. From half an ounce of this coal I got, in a gun-barrel, about an hundred ounce measures of air, three fourths of which was fixed air throughout, and the remainder inflammable; the first part of it burning with a bright white flame, like inflammable air from common pit-coal, the last part exploding like inflammable air from metals, only more faintly. Part of this air had probably come from the gun-barrel.

Bone ashes, I found, had not the same property of drawing fixed air from the atmosphere
that

that the ashes of vegetable and minerable substances have; but that the addition of spirit of nitre gives them that property.

SECTION IV.

Of the Generation of fixed Air from the vitriolic Acid.

I Had an evident proof of the generation of fixed air from the *vitriolic acid* united with spirit of wine, or with ether, which is produced from them both; so that these two acids, viz. the vitriolic and nitrous, agree in being capable of forming both dephlogisticated and fixed air.

After going through the process for making ether, from concentrated oil of vitriol and rectified spirit of wine, I had the curiosity to push the process as far as it would go, in order to examine whether any kind of air would be yielded in any stage of it. I therefore continued the distillation till the whole residuum was converted into a black mass, full of gross matter; and taking as much of the black lumps as filled about one fifth of an ounce

ounce measure, I put them into a tall glass vessel, and distilled them to dryness in a red hot sand heat.

The first air that came over was the common air a little phlogisticated, then the vapour of the watery part, and after that a large quantity of air, at first clear, but towards the middle of the process very turbid and white, but clear again at the last. I received in all about a pint and a half, in four portions, each of which contained about four fifths of fixed air, and the rest inflammable, burning with a blue flame; but the proportion of fixed air was something greater in the middle portions than either in the first or the last. I thought it possible that the cork, with which, as well as with clay and sand, the glass tube was joined to the glass vessel that contained the materials, might supply the inflammable air in part, as I perceived it was corroded and become black. It may be worth while to repeat this process in a glass retort.

Having gone over this process with spirit of wine, I recollected the black matter that was produced when I got vitriolic acid air from vitriolic acid and ether; and therefore determined to repeat that process and carry it farther; to see whether I should, in any part of it, get fixed air, as in the preceding experiment with the spirit of wine.

I therefore put one eighth part of vitriolic ether to a quantity of fresh distilled oil of vitriol, and

in a glass phial with a ground-stopper and tube, and with the heat of a candle, I got from it a great quantity of air, part of which was vitriolic acid air, which was absorbed by the water. But I observed, as the process advanced, the part that was not readily absorbed by water kept increasing, till at length the greater part of the produce was of this kind; and in the middle of the process it was very turbid. Examining this air it appeared to be fixed air, making lime water turbid, and being readily absorbed by water; but there was a residuum of phlogisticated air, about one sixth of the whole.

I then put the remaining materials, which were about an ounce measure, into a glass vessel; and with a sand heat I collected much more air than before, about two pints in all, the first part of which was the purest fixed air I had ever seen, having the smallest residuum. The last portion had more residuum, and this burned with a lambent blue flame. But this inflammable matter might possibly come from the cork with which the vessel was closed, as before; though I think it not so probable. At last the process was interrupted by an accident; but I concluded, from several circumstances, especially from the time that elapsed before the vapour ceased to issue from the orifice of the vessel (which continued buried in the
hot

hot sand) that more than twice the quantity of air might have been collected. The air had been very cloudy before the last portion, which contained the residuum of inflammable air.

From this experiment, especially that with the ether, in the glass phial and ground stopper, I think it is pretty evident, that fixed air is a *factitious substance*, and that the vitriolic, as well as the nitrous acid, may be converted into it.

SECTION V.

Of the Composition of fixed Air from dephlogisticated Air, and Phlogiston, by the Generation of it from heating together Substances containing each of them.

I Have several times given it as my opinion, that fixed air is a *factitious substance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that conclusion; but that it was composed of dephlogisticated air and phlogiston, though maintained by my friend Mr. Kirwan, I was far from being satisfied with, till I was forced to consent to his proof of it

from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on salts. But I have lately had two direct proofs of it by experiment.

The first experiment which seemed to prove that fixed air may be composed of dephlogisticated air and phlogiston, was made with *charcoal* and red precipitate, the charcoal being made with so great a degree of heat, that no fixed air could be expelled from it, not even when it was wholly dispersed by the heat of the sun in vacuo. This experiment is certainly, however, not so conclusive as the former; because, since dry wood and imperfectly made charcoal yield fixed air, it may be said that all the *elements* of this kind of air were contained in the most perfect charcoal. And though this substance alone will not, even with the assistance of water, give fixed air, it might be said, that this might be effected by its treatment with other substances, without their imparting any thing to it; especially as the inflammable air which is procured from charcoal by means of water appears to contain fixed air, when decomposed with dephlogisticated air. I think, however, that I have proved that this fixed air is really a composition of phlogiston contained in charcoal, and of the dephlogisticated air with which it was inflamed, the charcoal contributing nothing to it beside its phlogiston.

giston. In this place I shall only recite the facts concerning the production of great quantities of fixed air from perfect charcoal and red precipitate.

In order to expel all fixed air, I made a quantity of perfect charcoal from dry oak; and while it was hot I pounded it, and immediately mixing four measures of it with one of red precipitate, and putting them into an earthen retort, I presently got, in no greater a degree of heat than was necessary to revive the mercury, a large quantity of air, half of which was fixed air. Afterwards the proportion of fixed air was less, and towards the conclusion of the experiment there came no fixed air at all. This residuum was a little better at the first than at the last, when it was of the standard of 1.5.

As this air contained a greater portion of phlogisticated air than the common air of the atmosphere, and no spirit of nitre, or any thing that could yield spirit of nitre, was concerned in the experiment, it should seem that phlogisticated air may be composed of phlogiston and deplogisticated air; though this composition, according to the very capital discovery of Mr. Cavendish, may be reduced to spirit of nitre, or rather become one element in the composition of that acid.

In another experiment I hit upon a better proportion of the charcoal and red precipitate for

making pure fixed air. For mixing one ounce of red precipitate (which all chemists, I believe, are agreed to be the same thing with *precipitate per se*) and one ounce of perfect charcoal, fresh from the retort in which it was made; and putting them into a coated glass vessel, I procured from the mixture, by heat, about thirty ounce measures of air, the whole of which was the purest fixed air, leaving only about one fortieth part not absorbed by water, and this not inflammable, but of the standard of 1.7, or almost perfectly phlogisticated.

This experiment made me recollect those which I had formerly made with charcoal heated in nitrous acid, in which I had always procured a quantity of fixed air. I therefore repeated the experiment with some of the same charcoal which I had used in the preceding experiment, on the goodness of which I could depend; and I found that, when it was heated in the acid, in a glass phial with a ground stopper, it gave air, one fifth of which was fixed air. At another time I got air in this process, one half of which was fixed air. To the formation of this air, I presume, that the phlogiston from the charcoal and the dephlogisticated air, which is known to be produced by heating nitrous acid, must have contributed.

Being then apprized of the objection that might be made to the use of *charcoal*, as, notwithstanding
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the great heat with which it was made, containing at least the elements of fixed air, I made use of *iron*, to which no such objection could be made; and mixing an ounce of the red precipitate with an ounce of iron filings, and then heating them in a coated glass retort, I got twenty ounce measures of air, of which only one seventh remained unabsoꝛbed by water. The residuum was of the standard of 1.52, but slightly inflammable.

Again, from half an ounce of red precipitate, and half an ounce of iron filings, I got twenty six ounce measures of air, of which the first part was pretty pure fixed air; but afterwards one tenth of it remained unabsoꝛbed by water. Then, increasing the proportion of iron, I mixed one ounce of red precipitate with two ounces of iron filings, and got about forty ounce measures of air, of the first portions of which only one twentieth was unabsoꝛbed by water, though towards the conclusion of the process this residuum was greater. In this process I got, in the whole, thirty six ounce measures of pure fixed air, completely absoꝛbed by water, besides what was absoꝛbed both in the first reception of the air (which was in vessels containing water) and afterwards in transferring this air into those vessels in which the quantity of it was noted, the whole of which I suppose might be about four ounce measures more. Examining the first residuum of this pro-

cess by nitrous air, the standard of it was 1.6, and afterwards 1.7*.

Having heard that it was objected to this experiment, that iron contains a quantity of *plumbago*, and that the fixed air which I procured might come from that ingredient in it (though the quantity was certainly much too great to be accounted for in that way) I made use of other metals, to which no such objection could be made, viz. *brass* and *zinc*, and with the same result.

With two ounces of brass dust I mixed one ounce of red precipitate, and in a coated glass retort I got from it a quantity of air, two thirds of which was fixed air. The standard of the residuum was 0.6; so that there had been too great a proportion of the

* It appeared, in some of these experiments, that three ounce measures of dephlogisticated air go into the composition of two ounce measures of fixed air. For one ounce of this red precipitate gave sixty ounce measures of dephlogisticated air; and when mixed with two ounces of iron filings, it gave about forty ounce measures of fixed air that were actually absorbed by water, besides a residuum that was inflammable. I had the same proportion when I used half an ounce of each of the materials. But when I used one ounce of each, I got only twenty ounce measures of fixed air, including the residuum. At other times I had different proportions with different quantities of iron filings and charcoal.

It must be observed, however, that part of the fixed air is always imbibed by the water in which it is first received. Otherwise, in this experiment, the fixed air would have weighed no more than the dephlogisticated air in the composition of it, so that nothing would be left for the inflammable air.

red

red precipitate. But fixed air was produced in a quantity abundantly sufficient for my purpose.

In a coated glass retort, I put a mixture of one ounce of red precipitate and one ounce of filings of *zinc*, and got some air, part of which was clearly fixed air; but the retort very soon cracking, put an end to the experiment, and I did not think it necessary to repeat it. I imagine, however, that it will only be at the beginning of this process that much fixed air can be procured, unless more precaution be used in conducting it. For the neck of the retort breaking quite off, there issued from it a strong flame, which evidently arose from the burning of the *zinc* in the dephlogisticated air from the precipitate.

As turbeth mineral gives dephlogisticated air, as well as red precipitate, I mixed this substance with iron filings, and had a similar result, when I heated them together in an earthen retort. One ounce of the turbeth mineral with two ounces of iron filings, yielded about sixteen ounce measures of air, of which about one third was fixed air, and the rest of the standard of 1.5.

Another experiment which seems to prove the formation of fixed air from phlogiston and dephlogisticated air, is the expulsion of it from that *black powder* which is formed by the union of lead and mercury. This powder, I have observed, can only

be made in pure air, which is no doubt absorbed by the metals; and this being again expelled by heat, together with the phlogiston which had belonged to the lead, is that, I presume, which forms the fixed air that is found in this process.

When I began to make observations on this black powder, I mentioned my having expelled some fixed air from it. This was from such powder as I had found ready made; and therefore, not knowing with certainty what the composition of it was, I dissolved one ounce of lead in pure mercury, and then expelled it again in the form of this black powder, which, when the running mercury was pretty carefully pressed out of it, weighed about twelve ounces. Then exposing it to heat, in a coated glass retort, I got from it about twenty ounce measures of air, making allowance for the quantity of fixed air, which, as I supposed, might have been absorbed by the water, in receiving and transferring the air before any account was taken of the quantity of it. Of this air about one thirtieth part only was not absorbed by water. The residuum I did not examine. I must however observe, that in general, besides the fixed air, I obtained a considerable quantity of the purest dephlogisticated air, from this black powder.

In making the black powder that was used in the preceding experiment, I occasionally changed the
air

air in the phial, in which I shook the mercury, by blowing into it, sometimes with a pair of bellows, and sometimes with my mouth; and as it was suggested that this might have supplied the fixed air which I afterwards found in the black powder, I dissolved two ounces of lead in mercury, and got the black powder without blowing into the vessel at all, only changing the air so much oftner as was then necessary. From six ounces of the black powder thus carefully prepared, I expelled four ounce measures and a half of air, of which one and a half was pure fixed air. This was sufficient to satisfy me that *some* fixed air is certainly procured in this process. The residuum of this fixed air was of the standard of 1.7, or 1.8. I did not at this time get from this powder all the air it would have yielded.

Being now satisfied that there was no occasion to prepare this black powder with the precaution mentioned above, I repeated the experiment with ten ounces of it prepared in the readier method which I had used before, with a view to examine the residuum of the air, when the fixed air should be separated from it. The produce of air was in all about twenty three ounce measures, which I received in four portions of five ounce measures each, and another containing the remainder. All these portions I examined separately, observing the proportion

portion of residuum in each of them, and the quality, as measured by my usual standard, and the result was as follows. Of the first portion there remained one fourth, of the standard of 1.6; of the second one third, of the standard of 1.44; of the third one half, of the standard of 0.8; and of the fourth three fourths, of the same quality with the preceding.

In the last portion the residuum was one half of the whole, and that I found to be so pure, that, mixing it with two equal quantities of nitrous air, the standard of it was 0.63; so that the quality of these residuums was continually purer, till at the last it was pretty highly dephlogisticated.

It may be inferred from both these courses of experiments, that fixed air consists not of inflammable air (which I suppose necessarily contains water) but of pure phlogiston, and dephlogisticated air. In the experiments with the red precipitate and iron, no water at all is concerned, unless either the iron itself contain some, or the mercury, or dephlogisticated air: since when the red precipitate is decomposed by itself, nothing is produced besides mercury and dephlogisticated air, without any water. The experiment with the black powder will equally authorize the same conclusion, as neither the lead, the mercury, nor the pure air that combines with them, has been supposed to contain any water. It must how-

ever be observed, that the greatest part of dephlogisticated air is water.

While I am upon the subject of this black powder, I shall observe, that it occurred to me to mix with it more matter containing phlogiston, in order to see what change that would make in the residuum of the produce of air.

From four ounces of the black powder mixed with two ounces of iron filings, heated in an earthen retort, I expelled fifty four ounce measures of air, of which not more than four ounce measures were fixed air, and the residuum, examined at different times, was of the standards of 1.3, and 1.44; but the greater part of it was of 1.52, so that there was a considerable production of inflammable air from the iron. In this experiment, I raised the heat very gradually, till I had got one third of the produce of air. This I did from an idea that this moderate heat might increase the quantity of the fixed air, but it did not appear to make any difference in this respect.

Then varying the proportion of the ingredients, I mixed twenty ounces of the black powder with only one ounce of iron filings, and receiving the air in three portions, observed as follows. The first portion, which contained six ounce measures, had a residuum of 3.5, of the standard of 1.6. The second, which was one ounce measure, had a residuum

duum of 0.12. of the standard of 1.7; and the third portion, which was only one ounce measure, had a residuum of 0.12, of the standard of 1.7. Whether this was the whole of the produce of air, I do not recollect.

In order to try more fully the effect of different degrees of heat, I repeated the process with the black powder, only determining to suspend the process in the middle of the produce of air. Accordingly I heated two ounces of the black powder in a porcelain vessel; when I observed that some portions of the produce contained about one half fixed air, and that this proportion kept growing less and less, till the produce consisted of nothing but the purest dephlogisticated air, the standard of it being, with two equal measures of nitrous air, 0.2. I then let the vessel cool, and observed that, on resuming the experiment, the air came with the same purity to the last.

Examining the residuum in the retort, I found half an ounce of red powder, the colour of which could hardly be distinguished from that of precipitate *per se*. So that, no doubt, the mercury had been converted into it, and this very pure air was probably that which came from the precipitate as it was reviving. In this way, therefore, it would be easy to make this precipitate in large quantities, could a method be found of separating it from the
red

red lead, with which it is, in this process, necessarily mixed.

In the preceding experiment it will have been observed, that, at first, the residuum was considerably phlogisticated, but at the last remarkably pure. An accident in a subsequent experiment I once thought had discovered the cause of this difference. In the middle of one of the processes, in which I was using the black powder only, heating it in a glass vessel, a quantity of water was drawn up through the tube that communicated with the recipient, and got into the vessel that contained the black powder; and in all the remainder of that process, the residuum of the air was no better than about the standard of 1.7. Water came over along with this air to the very last, though the bottom of the vessel was red hot. When the process was over, the matter taken out of the vessel was still moist, and of a dark grey colour.

On this I made a paste of the powder with water, and drying it a little, immediately repeated the experiment with it; but I found no sensible difference between the substance in this state, and that which had not been wetted. Four ounces of it yielded 120 ounce measures of air, of which about twelve were pure fixed air, completely absorbed by water, and the rest highly dephlogisticated. However, in one process of this kind, from two ounces and a half of this powder, which had been
moistened

moistened and dried again, I got seventy ounce measures of air, of which only a very small part was fixed air, and the residuum was by no means pure dephlogisticated air. For with two equal measures of nitrous air, the standard was 1.2 and 1.3. At other times also I have had much less fixed air from this black powder when it had not been wetted, than in several of the instances above-mentioned; and I have not as yet been able to discover the circumstance on which the production of it in a greater or less quantity depends.

In the preceding processes with this black powder, I always got from it more or less of fixed air. But thinking to produce more of it by heating this substance with a burning lens in dephlogisticated air, I was surprised to find, that I only increased the quantity of dephlogisticated air in the vessel, and produced no fixed air at all. Whence this remarkable difference could arise, I do not pretend to say. It will be seen, that, in this process with inflammable air, I found it to be a matter of indifference whether I used this black powder or the red precipitate; both of them equally imbibing inflammable air, without producing either water or fixed air.

SECTION VI.

Of the Generation of fixed Air by heating Substances containing Phlogiston in dephlogisticated Air.

ANOTHER decisive proof of the generation of fixed air from phlogiston and dephlogisticated air, is the constant production of it when iron is melted in dephlogisticated air over mercury, by means of a burning lens. This experiment being a very pleasing one, I repeated it very often; and as it is on too small a scale to admit of great exactness, I shall mention the results of several of them, observing, in the first place, that no water is produced in this process.

In six ounce measures and a half of dephlogisticated air, I melted turnings of malleable iron till there remained only an ounce measure and one third, and of this twenty seven thirtieths of an ounce measure was fixed air. In six ounce measures of dephlogisticated air, of the standard of 0.2, I melted iron till it was reduced to two thirds of an ounce measure, of which one half was fixed air, and the remainder completely phlogisticated. Again, I melted

melted iron in seven ounce measures and a half of dephlogisticated air of the same purity with that in the last experiment, when it was reduced to an ounce measure and one third, and of this four fifths was fixed air, and the remainder phlogisticated. In this case I carefully weighed the finery cinder that was formed in the process, and found it to be nine grains; so that the iron that had been melted (being about two thirds of this weight) had been about six grains. I repeated the experiment with the same result.

When the dephlogisticated air is more impure, the quantity of fixed air will always be less in proportion. Thus having melted iron in seven ounce measures of dephlogisticated air of the standard of 0.65, it was reduced to 1.6 ounce measures, and of this only one third of an ounce measure was fixed air.

Prussian blue is generally said to be a calx of iron supersaturated with phlogiston, though of late it has been said by some, that it has acquired something that is of the nature of an acid. From my experiments upon it with a burning lens in dephlogisticated air, I should infer that the former hypothesis is true, except that the substance contains some fixed air, which is no doubt an acid. For much of the dephlogisticated air disappears, just as in the preceding similar process with iron.

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I threw the focus of the burning lens upon fifty three grains of Prussian blue, in a vessel of dephlogisticated air of the standard of 0.53, till all the colour was discharged. Being then weighed, it was twenty two grains. In this process seven ounce measures and a quarter of fixed air had been produced, and what remained of the air was of the standard of 0.94. Heating the brown powder to which the Prussian blue was reduced in this experiment in inflammable air, it imbibed eight ounce measures and a half of it, and became of a black colour; but it was neither attracted by the magnet, nor was it soluble in oil of vitriol and water, as I had expected it would have been.

Again I heated Prussian blue in dephlogisticated air of the standard of 0.2, without producing any sensible increase of its bulk, when I found three ounce measures of it to be fixed air, and the residuum tolerably pure, for, with two measures of nitrous air, the standard of it was 1.35. The substance had lost eleven grains, the greatest part of which was evidently water.

To determine what quantity of fixed air Prussian blue would yield by mere heat, I put half an ounce of it into an earthen tube, and got from it fifty six ounce measures of air, of which sixteen ounce measures were fixed air, in the proportion of one third in the first portion, and one fourth in the

last. The remainder was inflammable. There remained 140 grains of a black powder, with a very little of it, probably the surface, brown.

Comparing these experiments, it will appear that the fixed air procured by means of Prussian blue and dephlogisticated air, must have been formed by phlogiston from the Prussian blue and the dephlogisticated air in the vessel. For if 240 grains of this substance yield sixteen ounce measures of fixed air, ten grains of it, which is more than was used in this experiment, would have yielded 0.6 ounce measures. Nor is it possible to account for the disappearing of so much dephlogisticated air, but upon the supposition of its being employed in forming this fixed air.

In all the experiments with iron it cannot be doubted but that the greater part of the dephlogisticated air (*viz.* the water in it) incorporates with the iron, converting it into a *scale*, or *finery cinder*, being the very same substance with that which is produced by transmitting steam over iron when it is red hot; but at the same time some phlogiston must be expelled from the iron, and unite with the dephlogisticated air in the vessel, in order to form the fixed air that is found in it; as in other cases it unites with *water*, and makes inflammable air.

Perhaps as decisive a proof as any of the real production of fixed air from phlogiston and dephlogisticated

gified air, may be drawn from the experiments in which I always found a quantity of it when I burned sulphur in dephlogified air. In one of those experiments to which I gave more particular attention, six ounce measures and an half of dephlogified air were reduced to about two ounce measures, and one fifth of this was fixed air. Much vitriolic acid air had been produced in this process: For, before I admitted any water to it, the six ounce measures and a half were only reduced to six. When both the vitriolic acid and the fixed air were absorbed by water, the remainder was very pure dephlogified air, the standard of it being 0.3.

I had always concluded that no fixed air could be produced by the decomposition of inflammable air, which had been procured by means of the mineral acids, because I had not been able to do it with that which I had got by means of the vitriolic acid; but I learned from Mr. Methrie, that this is peculiar to the vitriolic acid, the remains of which, diffused through the inflammable air procured by it, he conjectures, may decompose the fixed air actually produced in the process. See his Treatise, p. 110. For, as I have hinted before, when the inflammable air is produced from iron, by means of spirit of salt, there is a very perceivable quantity of fixed air, when it is united with dephlogi-

ticated air. When I decomposed these two kinds of air in equal quantities, they were reduced to about 0.5 of a measure, and of this not more than about one fortieth part was fixed air. This experiment, ought, however, to be added to the other proofs of fixed air being produced by the union of the dephlogistified air and phlogiston.

The last instance of the generation of fixed air from phlogiston and dephlogistified air, which I shall mention in this section, is of a much more striking nature than any that I have yet recited. Having made what I call *charcoal of copper*, by making vapour of spirit of wine pass over copper when it was red hot, I took a piece of it, and, with no very particular view, heated it in different kinds of air. Among others, I did this in common air, and not observing any increase or decrease of the quantity of air, concluded, but too hastily, that no change was made in it. For when I repeated the experiment in dephlogistified air, the charcoal burned very intensely; and when a part of it was consumed (which, like common charcoal in the same process, was done without leaving any sensible residuum) I found that no heat which I could apply afterwards had any farther effect on what was left of the charcoal. Concluding, therefore, that some change must be made in the quality of the air, I examined it, and found about nine tenths

tenths of it to be the purest fixed air, and the residuum was such as would have been made by separating the absolutely pure part of the dephlogisticated air, and leaving all the impurities in what remained.

Having ascertained this fact, I repeated the experiment, weighing the piece of charcoal very carefully before and after the process, and then found that, by the loss of one grain of the charcoal, I reduced four ounce measures of dephlogisticated air till one ninth only remained unabsorbed by water; and again, with the loss of one grain and an half of the charcoal, I reduced six measures and an half of dephlogisticated air till five ounce measures and a half were pure fixed air.

In this process there was a diminution of the bulk of the air after the experiment, as might be expected from the change of the air into one of a heavier kind by means of a substance, or principle, that could not add much to the weight of it; but I did not accurately measure this. In one of the experiments 4.3 ounce measures of dephlogisticated air were diminished, I observed, about one thirtieth part of the whole. But being in a pretty wide vessel, such a measure cannot be accurate enough for computation. In this case, when the fixed air was separated by water, there was a residuum of 0.75 of a measure of the standard of

1.0, whereas the dephlogistified air before the experiment had been of the standard of 0.2.

That dephlogistified air actually enters into the composition of fixed air in this experiment, is evident from the weight of the fixed air, which far exceeds that of the charcoal, which is dispersed in the process. For in this last experiment the weight of the fixed air produced was 4.95 grains. Consequently, supposing the charcoal to be wholly phlogiston, as it is very nearly so, fixed air may be said to consist of 3.45 parts of dephlogistified air, and 1.5 phlogiston. So that the dephlogistified air is more than three times the proportion of the phlogiston in it.

I must not conclude this section without observing that, I never failed to produce fixed air, by heating iron in vitriolic acid air. I repeated the experiment many times, and always had this very remarkable result. In this case the acidifying principle, which is the chief ingredient in dephlogistified air, must have been supplied by the acid in the air.

In one of the experiments, four ounce measures of the vitriolic acid air were reduced to 0.65 of an ounce measure; and of this three parts and one half of the whole was fixed air, absorbed by lime water, and the remainder was slightly inflammable. In another experiment I could not perceive any thing
inflam-

inflammable in the residuum. It appeared to be only phlogisticated air. But these residuums are always small, so that it is not easy to distinguish weakly inflammable air from that which is phlogisticated.

SECTION VII.

Of the Production of fixed Air by heating Substances containing dephlogisticated Air in inflammable Air.

AS fixed is always produced when iron, or any other substance containing phlogiston is heated in dephlogisticated air; so when precipitate *per se*, or any other substance containing dephlogisticated air is heated in inflammable air, fixed air never fails to be procured.

In ten ounce measures of inflammable air from malleable iron I revived *red precipitate* till there remained only 1.1 ounce measure of air, and of this 0.07 ounce measures was fixed air, being completely absorbed by water. The weight of this air would be 0.063 gr. But, since 960 grains

of iron will yield 1054 ounce measures of inflammable air, the iron employed in procuring all the inflammable air that was used in this experiment, viz. 8.9 ounce measures (without allowing for any that went to the revivification of the mercury) would be 8.1 grains; and since M. Bergman supposes, that 100 grains of iron contains 0.12 grains of plumbago, the quantity of it in this iron would only be 0.01008 gr. which is not quite a sixth part of the weight of the fixed air.

With some *precipitate per se*, sent me by M. Berthollet, I revived mercury till eight ounce measures and a half of inflammable air was reduced to two ounce measures and a half, and of this 0.04 oz. m. at least was fixed air. This is not quite so much in proportion as in the preceding experiment, but abundantly more than the weight of the plumbago.

In eight ounce measures of inflammable air I revived *minium* (which I found to have exactly the same effect in this process as red precipitate, or precipitate *per se*) till it was reduced to 1.2 ounce measures; and of this 0.028 oz. m. was fixed air, which would exceed the weight of the plumbago more than three times. In reviving lead from massicot (which I prepared by expelling the pure air from minium) I had no fixed air in the residuum.

In

In seven ounce measures of inflammable air from tin by spirit of salt, I revived red precipitate till it was reduced to 1.1 ounce measure; and in this the fixed air was something more than in proportion to that in the last experiment.

I do not know that any objection can be made to the inflammable air from *tin*, as this metal has not been proved to contain plumbago. I wished, however, to repeat this experiment with inflammable air from *sulphur*. But though, when steam is sent over melted sulphur, a small quantity of inflammable air is procured; yet, as sulphur cannot part with much phlogiston, except in proportion as it imbibes pure air, to form oil of vitriol, I could not in this manner easily procure enough for my purpose.

In order to supply the sulphur with pure air, I mixed with it a quantity of *turbith mineral*; but this made it yield vitriolic acid air, though in great abundance, there not being, I imagine, *water* enough to form inflammable air: for when iron is dissolved in concentrated acid of vitriol, vitriolic acid air is produced; but in diluted vitriolic acid, the produce is inflammable air. With a view to supply these materials with water, I sent steam over them; but it did not combine with the air, which was still only vitriolic acid air.

Since,

Since, however, vitriolic acid air unquestionably contains the same principle which forms the inflammability of inflammable air, this experiment proves that sulphur is not that simple substance which the antiphlogistians suppose it to be; but that it contains phlogiston. Had it been nothing more than a substance which had a strong affinity to pure air, it would have united with the pure air from the turbith mineral, and have made vitriolic acid; but no vitriolic acid air would have been produced.

That vitriolic acid air contains the same inflammable principle with inflammable air is evident from the quantity of vitriolic acid air which I produced by reviving copper from blue vitriol in inflammable air. Mr. Kirwan also produced this air from sulphur and red precipitate. See his Treatise on Phlogiston, p. 29.

When I used a small quantity of sulphur in proportion to the turbith mineral, the first produce was vitriolic acid air, and afterwards dephlogisticated air, from the turbith mineral alone, the effect of the sulphur-having been exhausted.

According to the antiphlogistic theory, *phosphorus*, as well as sulphur, is a simple substance; and when it is ignited imbibes pure air, and thereby becomes the phosphoric acid, without parting with any thing. But I find, that after the accension of
it

it in dephlogisticated air, there is a considerable quantity of fixed air in the residuum; and this fixed air could only be formed by the union of the dephlogisticated air in the vessel with the phlogiston contained in the phosphorus. Mr. Kirwan had a similar result from phosphorus confined in atmospheric air. As it is not pretended, that there is any plumbago in phosphorus, this experiment is not liable to the objection that has been made to those in which inflammable air from iron was made use of.

Comparing this experiment with that in which iron is ignited in dephlogisticated air, and those in which nitrous acid is produced by the accension of dephlogisticated and inflammable air, this general conclusion may be drawn, viz. that when either inflammable or dephlogisticated air is extracted from any substance in contact with the other kind of air, so that one of them is made to unite with the other in what may be called its *nascent state*, the result will be *fixed air*; but that if both of them be completely formed before their union, the result will be *nitrous acid*.

It has been said, that the fixed air produced in both these experiments may come from the *plumbago* in the iron from which the inflammable air is obtained. But since we ascertain the quantity of plumbago contained in iron by what remains after

its solution in acids, it is in the highest degree improbable, that whatever plumbago there may be in iron, any part of it should enter into the inflammable air procured from it. Besides, according to the antiphlogistic hypothesis, all inflammable air comes from water only.

In the course of these experiments I discovered more completely than before the source of my former mistake, in supposing that fixed air was a necessary part of the produce of red lead, and also of manganese. Both these substances, I find, give of themselves only dephlogisticated air, and that of the purest kind; and all the fixed air they yielded in my former experiments must have come from the gun-barrel I then made use of, which would yield inflammable air, which, with dephlogisticated air, forms fixed air. For though the dephlogisticated air from red lead was so pure that, mixed with two measures of nitrous air, the three measures were reduced to five hundredth parts of a measure, and the substance gave no fixed air at all when it was heated in an earthen tube or retort; yet by mixing iron filings with it, or with manganese, as I had formerly done with red precipitate, I got more or less fixed air at pleasure, and sometimes no dephlogisticated air at all.

I cannot conclude these observations without taking notice, how very valuable an instrument in
philo-

philosophy is a good burning lens. This must have been perceived in many of my former experiments, but more especially in these. By no other means can heat be given to substances *in vacuo*, or in any other kind of air besides atmospherical; and without some method of doing this, no such experiments as these can possibly be made. I therefore congratulate all the lovers of science on the successful attempt of Mr. Parker to execute so capital an instrument as he has done of this kind. Such spirited and generous exertions reflect honour on himself, and on our country. It is only to be wished, that we could have lenses of a smaller size (*viz.* from twelve to eighteen inches diameter) made tolerably cheap, so that they might be in more common use. All the preceding experiments were made with one of twelve inches in diameter.

SECTION VIII.

Of Air acting through a Bladder.

AS it decisively follows from my experiments on the action of different kinds of air through a bladder, that fixed air consists of dephlogisticated air and phlogiston, I shall introduce them in this place.

One of my former experiments which I was least able to account for, was the diminution of nitrous air in a bladder swimming at liberty in a trough of water; the consequence of which had always been, that in a few days the nitrous air was diminished about one fourth, and this was phlogisticated air. All the progress that I had then made in the investigation of this curious fact, was finding that it depended, as I then thought, upon the bladder being kept alternately dry and moist; because when the bladder was kept covered with water, it remained full, and the air within it was not changed. This was also the case when the bladder was kept dry. But I did not consider that when the bladder was kept under water, there was

no *air* in contact with it; and I did not then suspect that this change in the air depended on the action of the nitrous air upon the external common air through a moist bladder; though I had found that coagulated blood has a power of acting upon air, and is of course liable to be acted upon by air, through any bladder.

At length, suspecting that this *might* be the case, I made the following experiment. Taking a bladder which contained twenty ounce measures of nitrous air, and tying it very tight, I introduced it into a glass jar, which contained forty ounce measures of common air; because, in that proportion, they would be able, if they had any mutual action, to saturate one another. Wishing at the same time, to observe the changes that might gradually take place in each of the kinds of air, I examined them both at different periods.

The process was begun on the 18th of May, and on the 21st I found that there were only thirty four ounce measures of the common air, and eleven of the nitrous, the bladder being quite sound; so that it was sufficiently evident, that the two kinds of air, had affected each other through the substance of the bladder. On the 25th of the same month there were thirty one ounce measures and a half of the common air, and four and a half of the nitrous; and examining the state of both of them,

them, I found the standard of the common air to be 1.8, which was a state very near that of extreme phlogistication; and that of the nitrous 1.7. That is, equal measures of this and of common air, occupied the space of 1.7 measures, which shews that it had almost lost its power of affecting common air, or to express myself perhaps more correctly, there was but a small proportion of nitrous air in it.

On the 8th of June I examined them for the last time, after having observed no farther change for some days in the quantity of the common air (as indicated by marks which I had made on the outside of the jar) and I found only twenty eight ounce measures of the common air, of the same quality as when I had examined it before, viz. of the standard of 1.8, and only three ounce measures of the nitrous air, and it did not affect common air at all. Neither of them contained any portion of fixed air, and both of them extinguished a candle.

Nothing now remained to my complete satisfaction, with respect to my former observation of the diminution of nitrous air, contained in a bladder. But I farther wished to satisfy myself with respect to the action of *inflammable air*, on either common or dephlogisticated air, in the same circumstances. Nitrous air affects pure air by simple contact,

contact, without ignition; whereas, inflammable air, I had observed, has very little effect upon pure air when they are simply mixed together. I was, therefore, surprized to find that inflammable air has a very considerable action upon dephlogisticated air through a bladder, without any assistance from heat; and moreover, that the union of these two kinds of air, thus produced, forms fixed air. The experiments which I made for this purpose, were as follows.

Into a jar containing 123 ounce measures of dephlogisticated air, I introduced a bladder, containing twenty three ounce measures of inflammable air; and after a few days, I observed that the bladder in which it was contained was become a little flaccid. After about three weeks, I examined both the kinds of air, and found that the bladder contained only two ounce measures, and that this was no longer inflammable, but extinguished a candle, though it had in it a mixture of pure air. The air within the jar then contained one twentieth of its bulk of fixed air. The dephlogisticated air was diminished seven ounce measures; and from being of the standard of 0.5, with two equal measures of nitrous air, it was now become of 1.4. The bladder had a slight smell of putrefaction, but it was perfectly air tight.

It is observable, that in this experiment part of the dephlogisticated air had passed unchanged into the bladder of inflammable air, whereas the inflammable air which had passed through the bladder into the dephlogisticated air, had united to it, and formed fixed air. The transimission of the dephlogisticated air through the bladder was much more remarkable in the following experiment.

Having introduced a bladder filled with inflammable air into a large jar of dephlogisticated air, the bladder, after two days only, had in it a great mixture of dephlogisticated air, and was as much distended as when it was first put into the jar. A quantity of it exploded exactly like a mixture of one third dephlogisticated, and two thirds inflammable air. The bladder was perfectly sound and sweet, and the dephlogisticated air was not sensibly altered.

Again, having introduced a bladder containing ten ounce measures of inflammable air into a jar containing one hundred ounce measures of dephlogisticated air, of the standard of 0.3, I found, about a month afterwards, that the air in the jar was diminished to ninety ounce measures, and the inflammable air to five ounce measures and an half. The quality of the air in the bladder and of that in the jar was very nearly the same, though the bladder was perfectly sound and sweet. The air in
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the bladder, with equal measures of nitrous air, was of the standard of 0.76, and that in the jar of 0.74. Both of them also contained a small portion of fixed air. In this case, therefore, both the kinds of air had not only been transmitted through the bladder, but some decomposition had also taken place within it, as well as within the jar.

In another experiment of this kind, both the bladder of inflammable air, and the jar of dephlogisticated air, after some time, contained each of them a portion of fixed air, and likewise both the kinds of air unaffected by each other. For both of them exploded when they were examined separately.

It seems to follow from these experiments, that fixed air is really *formed* when inflammable air of charcoal, &c. is exploded together with dephlogisticated air; and also that the greatness of the heat prevents its formation, when inflammable air from metals is used. For though, in the explosions with the electric spark, no fixed air was produced from the decomposition of the purest inflammable air, it was evidently so with the same kind of inflammable air in these experiments with a bladder in which no heat is used.

The formation of fixed air from phlogiston and dephlogisticated air, is more evident from the great quantity of it which is found when an animal sub-

stance putrefies in dephlogisticated air, compared with the small quantity that is procured by its putrefying in inflammable air.

After the preceding experiments on the consequence of having one kind of air in the bladder, and the other in the jar in which it was confined, I filled the bladder with the same air that was in the jar, and let them remain till they became putrid and burst. The jar and the bladder of dephlogisticated air contained together one hundred ounce measures of the standard of .95, but after the process and washing the air in water there were only 37.5 ounce measures, which was phlogisticated.

At another time ninety ounce measures of dephlogisticated air of the standard of 0.16, were reduced to forty seven ounce measures of the standard of 0.6; whereas a jar of inflammable air of the same size, and treated in the same manner, contained, after the process, not more than one thirtieth of its bulk of fixed air. In this it was observable, that the bladder and the air were most abominably offensive, whereas the bladder which had been in dephlogisticated air was hardly offensive at all.

It will appear by computation, that in both these cases of the formation of fixed air, by the bladders putrefying in dephlogisticated air, phlogisticated

gified air was produced, *six* ounce measures being generated in the former case, and *five* in the latter; and though all fixed air contains a part not absorbed by water, and this is always more or less phlogified, this was much more than in that proportion, the phlogified air being in the former case one sixth of the whole, and in the latter nearly one half. For in the former case the phlogified air before the process was 31.7 ounce measures, and after it 37, and in the latter it was 4.86 ounce measures before, and 9.4 after.

B O O K II.

EXPERIMENTS AND OBSERVATIONS RELATING TO INFLAMMABLE AIR.

P A R T I.

EXPERIMENTS AND OBSERVATIONS RELATING TO THE PRODUCTION OF INFLAMMABLE AIR.

S E C T I O N I.

Of inflammable Air from Metals, by Means of Acids, &c.

THE metals from which this species of air has been procured are *iron*, *zinc*, and *tin*. I found it in *copper*, and *lead* by spirit of salt, as may be seen in the account of the discovery of marine and air. I have also procured it in various other ways; and have lately found that *regulus of antimony* dissolved in marine acid, with the application

cation of heat, yielded a small quantity of air, which was weakly inflammable. *Bismuth* and *nickel* were dissolved in marine acid with the help of a considerable degree of heat, but little or no air was got from either of them. If there was any more than the common air which had lodged within the phial containing the mixture, I could not perceive that it was inflammable: but these metals treated in this manner yielded a strong smell of *liver of sulphur*.

It is something remarkable, that all the acids that produce any air by the solution of metals give inflammable air, except spirit of nitre only, which forms a different kind of union with the inflammable principle; making *nitrous air*, more or less modified. Besides oil of vitriol and spirit of salt, I have observed that the *vegetable acid* also produces inflammable air, by the solution of metals, though in a much less quantity. Perhaps the proportion of the strength of the acids may be ascertained by this means. The concentrated vinegar which I made use of in my experiments on the vegetable acid air, dissolved zinc almost as rapidly as spirit of salt, and produced inflammable air; and *radical vinegar*, which is unquestionably a pure vegetable acid, had the same effect when applied both to zinc and iron.

In order to measure the strength of this acid, I put as much radical vinegar as occupied the space of fifty two grains of water upon a quantity of filings of zinc diluted with water, and found that it yielded one fourth of an ounce measure of inflammable air, without heat; and two ounce measures more with heat; and a little more might have been procured, if care had been taken that no part of the liquor had boiled over. What proportion this produce of inflammable air bears to a similar produce from spirit of salt may be found by comparing this observation with some that are mentioned relating to marine acid air.

In my first experiments on fixed air, I found that, when a mixture of iron filings and brimstone, moistened with water, was made to ferment in it, a part of it was made immiscible with water, that is, that there was in it a greater residuum of phlogisticated air than usual, which I supposed to come from the phlogiston set loose in this process; though I could not find that phlogiston in any other process produced that effect. At that time it could not but occur to me, that, possibly, this mixture itself might generate air, in which case the fact I have been reciting would not prove that there had been any alteration in the constitution of the fixed air; since there would have been a real *addition* to it, of another kind of air from the mixture. To
try

try this, I then made this mixture to ferment under water, and found that no air whatever was produced from it.

I have since tried the same thing in the best *vacuum* that I could make with Mr. Smeaton's air pump; when, though the fermentation went on as usual, yet when water was admitted to it afterwards, no air was found in the receiver. I also made this fermentation when the materials were buried in quicksilver, and in these circumstances also no air was produced in the temperature of the atmosphere.

I mention these circumstances, because I have found that when this fermentation is made in quicksilver, and *in a warm place*, a true inflammable air is generated. The experiment was made in as accurate a manner as I could contrive, and in the course of it, it will be seen that probably a quantity of vitriolic acid air was also generated, and absorbed again by the water that was mixed with the iron and brimstone, and which is necessary to enable them to act upon each other.

Having filled a small phial with a mixture of iron filings and brimstone moistened with water, I plunged it in a vessel filled with quicksilver, standing inverted in a basin of the same, and placed the whole apparatus near the fire. In about half an hour the fermentation began, and so much air
issued

issued from the mixture, as occupied the space of four times the bulk of the materials. In a few minutes the quantity of air diminished, being probably vitriolic acid air, and having been absorbed by the water; but there remained about one fourth of the bulk of the mixture that was permanent air, not imbibed by water; and this was inflammable.

Since zinc, as well as iron, yields inflammable air with oil of vitriol, I suspected that possibly it might be affected as iron is by the oil of vitriol set loose from the sulphur in this process, and I found that when I substituted filings of zinc for the filings of iron, in the circumstances above-mentioned, they answered equally well. In this experiment a quantity of air was produced equal to the bulk of the materials, all strongly inflammable.

Having once put a pot of iron filings and brimstone into a jar of nitrous air (the first effect of which is to reduce it to one fourth of its bulk, and leave it in the state of phlogisticated air) and having some time after this found the air much increased in quantity, and strongly inflammable, I had some doubt whether the inflammable matter came from some farther change in the nitrous air, or from an exhalation of proper inflammable air from the iron and brimstone. My doubt arose from

from my never having found that this paste of iron filings and brimstone, whether kept in water, or in vacuo, had yielded air at any time, except in a considerable degree of heat. In consequence, however, of repeated experiments, I am now satisfied, that the inflammable air came from this mixture. For though some pots of it have not yielded inflammable air, they have all, with *long keeping*, even in the temperature of the atmosphere, yielded either phlogisticated or inflammable air; the latter generally when the composition was fresh made, and the former when it was old.

These experiments have also led me to the observation, that, in this and many other cases of the diminution of common air by phlogistic processes, a true inflammable air is first produced, and in its *nascent state*, as it may be called, is immediately decomposed, previous to the phlogistication of the common air. The very same substances which, in water or quicksilver, yield inflammable air, only phlogistificate common air: so that I am almost ready to conclude universally, that air is never phlogisticated, but by materials which, in certain circumstances, would yield inflammable air; though when inflammable air is previously produced, and then mixed with common air, it will not be decomposed in the temperature of the atmosphere, except in a very small degree. These two kinds
of

of air will, therefore, continue mixed without much affecting each other, except in a red heat, by which the inflammable air is fired. It is then well known to cease to be inflammable air, the phlogiston being separated from it, and uniting with the dephlogisticated air in the common air; when nothing is left but the phlogisticated part of the common air, which is about three fourths of the whole. I have since observed that *nitrous acid* is formed in these circumstances.

The experiments which led to these conclusions, and which I shall now proceed to recite, may serve as a caution to myself and others, not to be too hasty in drawing general conclusions; since what may appear to be the *same materials*, and the *same preparation* of them, may have different results, in consequence of there having been some circumstance, respecting either the materials or the process, that was unnoticed, but which was the secret cause of the unexpected results.

That nitrous air might be changed into inflammable air, was not extremely improbable *a priori*; since I had found that it contained nearly as much phlogiston as inflammable air, bulk for bulk; and since it is, by several processes, convertible into what has the appearance of a species of inflammable air. Besides, in this very case, the same composition of iron filings and brimstone, which I now
find

find generally yields inflammable air in the temperature of the atmosphere, does not do so at all times.

Thinking that if the iron filings and brimstone had really yielded the inflammable air which I found in the vessel of nitrous air, it would do the same in common air, I confined a large pot of this mixture in a very small quantity of common air in the beginning of February, 1779. But though on the 19th of May following it was increased in bulk, it was all mere phlogisticated air, and had nothing inflammable in it. Even the air that was entangled within the cavities of this pot of iron filings and brimstone, and which I caught by breaking it under water, was not inflammable. It is possible, however, as I observed before, that this phlogisticated air might have been inflammable air in its origin, or *nascent state*, and have become phlogisticated air afterwards. At another time I put a pot of this mixture under water, as I had done formerly, and now also observed, that though it fermented very well, and turned black, yet it did not yield a particle of air in about a fortnight: and in experiments of this kind few persons, I believe, would look for any farther change beyond that time.

Soon after, however, I found that a pot of this mixture, fresh made, and kept under water three weeks, had yielded about its bulk of air; and
this

this was strongly inflammable. But at the same time another mixture of this kind, kept in the same circumstances, yielded only phlogisticated air; and yet I did not knowingly make any difference in the composition, always mixing equal bulks of the two ingredients.

As the phlogiston which constituted the inflammable air in the experiments that occasioned these must probably have come from the iron, and not from the sulphur; especially since iron alone is capable of making a very remarkable change in nitrous air, I confined a quantity of this air, in a vessel full of iron nails, from the beginning of February to the 18th of May; but after this long interval it was only phlogisticated air, and not in the least inflammable.

Having found, however, that this mixture of iron filings and brimstone was capable of producing inflammable air in water, I made a trial of it in quicksilver, and found it to have the same effect. For confining a quantity of this mixture in quicksilver from the 13th to the 30th of June, in the temperature of the atmosphere, it had yielded, in this time, its own bulk of air, strongly inflammable.

I found afterwards, in a proper number of trials, that in a sufficient space of time, this mixture increased all the kinds of air into which I introduced

duced it, by the addition of a quantity of inflammable air, more or less, according to circumstances, known or unknown. But when the experiment was made in common air, it first diminished it about one fourth, as I have often noted; and some time after that I perceived an addition made to the bulk of the air, and examining it, found it at first to be slightly inflammable, but afterwards more strongly so. This experiment shews that, in the first instance, the inflammable air yielded by iron filings and brimstone must have been decomposed by uniting with the dephlogisticated air in the common air.

It appeared upon one occasion, recited above, that one pot of this mixture, *fresh made*, produced inflammable air, at the same time that a pot of an *old* mixture of this kind yielded only phlogisticated air. But at what time these mixtures will cease to give inflammable air, and begin to yield phlogisticated air, I cannot determine. For I find that on the 23d of June a pot of iron filings and brimstone, which must have been mixed about a year before, confined in a small quantity of common air, had made an addition to it of three ounce measures on the 26th of July; and this air was inflammable. At the same time I found that another quantity, which had been mixed the 1st of July, had yielded inflammable air, in about the

same proportion, according to the time. Also some old iron filings and brimstone, which had been taken out of the pot, and mixed with water the 3d of July, had yielded about one tenth of its bulk of air on the 2d of August, strongly inflammable.

That future experimenters may form some idea of the quantity of inflammable air that they may generally expect from such mixtures as I have usually made of iron filings and brimstone, using equal bulks of each, and therefore be less apt to deceive themselves in the results, I shall recite the issue of some that I made with this and other mixtures, and which I was obliged to put an end to when I removed my habitation on the 21st of July, 1780.

A gallipot, containing an ounce measure and half of this mixture, having been confined, in a small quantity of common air in the beginning of July, 1779, had at the time above-mentioned produced fourteen ounce measures of air, strongly inflammable; but the production was much more rapid at the first than afterwards. The mixture was very hard.

Another gallipot of the same size, put into a vessel of water, without any air, on the 23d of June, 1779, had three ounce measures of inflammable air taken from it on the 26th of July following,

lowing, and at this time there were eleven ounce measures, strongly inflammable. The mixture was very soft.

Another equal quantity had yielded strong inflammable air from the 24th of June to the 15th of July, 1779, and had from that time yielded about three ounce measures of air, but slightly inflammable. The mixture was very soft.

There is the same uncertainty attending experiments made with *liver of sulphur*, which also exhales phlogiston, and produces the same effect both on common air and nitrous air, as iron filings and brimstone. On the 19th of May, 1779, I found a quantity of nitrous air, in which some liver of sulphur had been confined from the 12th of December preceding, and which was considerably increased in bulk, to be strongly inflammable; and yet another quantity of this substance, and fresh made, was confined in quicksilver several months without producing any air at all.

I have procured inflammable air, in a considerable quantity, by dissolving iron filings in a solution of galls; and very probably the same would be produced by means of any other astringent substance. Indeed most things that really *decompose* the metal, and do not unite with the *whole mass* of it, will, I imagine, set loose the phlogiston it contains, in the form of inflammable air; though, in

several of the cases, the phlogiston might join some of the principles in the menstruum, and contribute to compose a different substance.

I was led to this observation of the production of inflammable air by the solution of galls, in consequence of being informed by Mr. Delaval, that ink might be made by putting iron to the solution of galls; for that the acid in the vitriol, which is commonly used for the purpose of making ink, is an unnecessary, and frequently an inconvenient ingredient.

Having mixed a quantity of pounded galls, iron filings, and water, I first observed, that, after a day or two, the whole mass was very much swelled; and that it was full of bubbles of air, which at the surface were very large. Suspecting, from the smell, and other circumstances, that the air contained in them was inflammable, I burst several of them near the flame of a candle, and found that they all made small explosions, so that I could have no doubt concerning the quality of the air.

I then mixed three ounces of pounded galls with water and iron filings, the quantity of which I did not note; and covering them with a large jar full of water, found that, in about a week, they had produced six ounce measures of air, which was strongly inflammable, exactly like that which is produced from iron by the acids. In the same

manner

manner I procured a quantity of this inflammable air by putting the above-mentioned mixture into a phial with a ground stopper and tube. But this process is too slow for any use.

SECTION II.

Of inflammable Air from Oil.

THE electric spark taken in any kind of oil produces inflammable air, as I was led to observe in the following manner. Having found, as will be mentioned hereafter, that ether doubles the quantity of any kind of air to which it is admitted; and being at that time engaged in a course of experiments to ascertain the effect of the electric matter on all the different kinds of air, I had the curiosity to try what it would do with *common air*, thus increased by means of ether. The very first spark, I observed, increased the quantity of this air very considerably, so that I had very soon six or eight times as much as I began with; and whereas water imbibes all the ether that is put to any kind of air, and leaves it without any visible

change, with respect to quantity or quality, this air, on the contrary, was not imbibed by water. It was also very little diminished by the mixture of nitrous air. From this it was evident, that it had received an addition of some other kind of air, of which it now principally consisted.

In order to determine whether this effect was produced by the *wire*, or the *cement* by which the air was confined (as I thought it possible that phlogiston might be discharged from them) I made the experiment in a glass syphon, and by that means I contrived to make the electric spark pass from quicksilver through the air on which I made the experiment, and the effect was the same as before. At one time there happened to be a bubble of common air, without any ether, in one part of the syphon, and another bubble with ether in another part of it; and it was very amusing to observe how the same electric sparks diminished the former of these bubbles, and increased the latter.

It being evident that the *ether* occasioned the difference that was observable in these two cases, I next proceeded to take the electric spark in a quantity of ether only, without any air whatever; and observed that every spark produced a small bubble; and though, while the sparks were taken in the ether itself, the generation of air was slow, yet

yet when so much air was collected, that the sparks were obliged to pass through it, in order to come to the ether and the quicksilver on which it rested, the increase was exceedingly rapid; so that, making the experiment in small tubes, as fig. 16, Pl. I. the quicksilver soon receded beyond the striking distance. This air, by passing through water, was diminished to about one third, and was inflammable.

One quantity of air produced in this manner from ether I suffered to stand two days in water, and after that I transferred it several times through the water, from one vessel to another, and still found that it was very strongly inflammable; so that I have no doubt of its being genuine inflammable air, like that which is produced from metals by acids, or by any other chemical process.

Concluding that the inflammable matter in this air came from the ether, as being of the class of oils, I tried other kinds of oil, as *oil of olives*, *oil of turpentine*, and *essential oil of mint*, taking the electric spark in them, without any air to begin with, and found that inflammable air was produced in this manner from them all. The generation of air from oil of turpentine was the quickest, and from the oil of olives the slowest in these three cases.

By the same process I got inflammable air from *spirit of wine*, and about as copiously as from the

essential oil of mint. This air continued in water a whole night, and when it was transferred into another vessel was strongly inflammable.

By the same process I got inflammable air from the *volatile spirit of sal ammoniac*; and as I have observed before, the alkaline air which is expelled from the spirit of sal ammoniac is inflammable.

Endeavouring to procure air from a caustic alkaline liquor, accurately made for me by Mr. Lane, and also from spirit of salt, I found that the electric spark could not be made visible in either of them; so that they must be much more perfect conductors of electricity than water, or other fluid substances.

In all these cases it is probable that the electric spark only gave the substances the degree of *heat* that was necessary to give the phlogiston, and the water they contained the form of permanent inflammable air; for this was done much more effectually by a direct application of heat in the experiments recited in the next section.

Inflammable air will sometimes issue spontaneously from oil of turpentine. I once opened a pint phial, half filled with this kind of oil, and the cork being very tight, there rushed out of it a great quantity of air; when applying the flame of a candle to the mouth of the phial, I found the remainder to be strongly inflammable. The oil was then quite full of air bubbles, and by the heat of boiling

boiling water I expelled from a quantity of it an equal bulk of air, all strongly inflammable, like that which is obtained from metals. It was eight or ten hours in giving this air. When I could perceive the colour of the flame, I found it to be blue.

I then took a quantity of the same kind of oil, which had been kept in another phial, but I found the air incumbent upon it, within the phial, to be only common air; but making it boil in a retort, I expelled from it twice its bulk of air, all strongly inflammable. I could not distinguish the colour of its flame.

When I had thus expelled all the air which a quantity of this oil of turpentine seemed to contain, I agitated it very strongly, and frequently, in the course of two days, in order to make it imbibe more air, that I might expel it again; but I did not find that it had imbibed more than a very small quantity, and this, when it came out again, was only common air slightly phlogisticated. The first boiling had made it brown, and very viscid.

SECTION III.

Of the Production of inflammable Air from different Substances, by means of Heat and Water.

IT is probable, that every substance which contains *phlogiston* may be made to yield inflammable air. But for this purpose they require different modes of treatment, according to their respective natures. If the substances be *fluid*, heat applied to them directly makes no change in their constitution; but when they are made to pass, in the form of *vapour*, through tubes previously made red hot, in which they are necessarily exposed to a red heat themselves, they are readily decomposed; and the quantity of inflammable air that was yielded by some of them, in this mode of treatment, appeared to me rather extraordinary.

I began these experiments with *spirit of wine*, having an apparatus proper to receive any *water*, or other fluid, that might be formed, or condensed, in the process, Pl. VII. fig. 2. From two ounce measures of spirit of wine, which was made to pass in vapour, through a red hot earthen tube, I got about 1900 ounce measures of air, which was all inflammable, without

without any mixture of fixed air in it, and which burned with a lambent blue flame. Thirty ounce measures of this air weighed eight grains less than an equal quantity of common air. In this process I collected 0.35 of an ounce measure of water.

In this experiment the air would have weighed

633 grains
the watery residuum 168
801

and the spirit of wine would have weighed 821; so that the produce was pretty nearly what might have been expected from the materials, the nature of the process considered.

I then proceeded to subject to the same process a quantity of *vitriolic ether*; and making an ounce measure of it pass through the hot earthen tube, almost filled with pieces of broken retorts, or crucibles (in order to make a greater quantity of red hot surface) I collected one tenth of an ounce measure of water, and 740 ounce measures of air, all inflammable, without any mixture of fixed air. It burned with a large lambent white flame, like that of wood in a common fire, and would not explode with any mixture of dephlogisticated air. Twenty-nine ounce measures of this air weighed five grains less than an equal bulk of common air.

In the next place, I made some vapour of *spirit of turpentine* pass through the hot earthen tube, and procured

procured from it a quantity of inflammable air, that was very turbid, like black smoke. But the black matter contained in it was soon deposited on the surface of the water in which it was received. This also contained no fixed air, and burned with a lambent flame, but much less luminous than that in the preceding experiment. The smell of this air was so exceedingly offensive, that, the apparatus being a little deranged, I discontinued the process before I had ascertained the quantity of air, and without collecting any water, which I suppose would have been given. Thirty ounce measures of this air weighed eight grains less than an equal quantity of common air.

I did not repeat this experiment with *olive oil*, being apprehensive that the process would be even more offensive than that with the spirit of turpentine, and nothing material depending upon it. But, upon another occasion, I mixed an ounce of olive oil with 874 grains of *calcined whiting*; and subjecting it to a red heat in an earthen retort, I got from it near 300 ounce measures of air, and should probably have got much more, if there had been more whiting in proportion to the oil. The first portion of this air burned with a large white flame, and the last with a slight lambent blue one, exactly resembling the varieties in the process for extracting air from wood; so that there can be no doubt,

doubt, but that it is the *oil* in the wood that gives the air. That excellent philosopher, Mr. Volta, was the first who hit upon this method, or a similar one, of getting inflammable air from *oil*; and he has given a large account of its peculiar properties.

From other experiments that I made, it appears, that *water* is essential to the formation of inflammable air. In all the liquid substances mentioned above, the water that enters into their composition is sufficient for the purpose; and spirit of wine, and ether, appear to contain more water than is necessary. But when the substances are *dry*, and water does not enter as a necessary ingredient into their composition, water must be introduced into the process. This is the case with all the *metals*, and it is no less so with *sulphur*, *arsenic*, and probably other substances of a similar nature, which mere heat only sublimes.

Transmitting steam over a quantity of *sulphur*, which was melting in a hot earthen tube, I procured from it a quantity of inflammable air, without any fixed air; and by analysis it appeared to be of the same quality with that which is procured from iron by oil of vitriol. This process is rather troublesome, on account of the sulphur subliming, and filling up the tubes through which the air is conveyed.

I then

I then repeated the same process with *arsenic*, and from this substance I procured air in great plenty. One seventh of it was fixed air, but the rest strongly inflammable, and the smell of it could not be distinguished from that of phosphorus. Twenty ounce measures of this air weighed four grains and a half less than an equal quantity of common air. This experiment was no less troublesome than the preceding, on account of the arsenic subliming, and choking up the tubes.

Having found a very heavy kind of inflammable air by heating the *scales of iron* mixed with *charcoal*, I made the following experiment in order to ascertain the quantity of air that might be procured from a given quantity of these materials. Mixing two ounces of the *scales*, or *finery cinder* (which I found to be the same thing) with one ounce of perfect charcoal, I got from it, in an earthen retort, 580 ounce measures of air, one tenth of the first part of which was fixed air; but afterwards it was all inflammable. The substances were pretty firmly concreted together, and weighed 1044 grains; so that the loss of weight was 396 grains, which must have been very nearly the weight of the air procured. Forty ounce measures of this air, freed from all fixed air, weighed two grains more than an equal quantity of common air.

Besides

Besides the *water*, which seems to be essential to the constitution of inflammable air, this species of air readily imbibes more water, which adds greatly to its specific gravity. This seems at least to be indicated by the following experiment. Filling a dry bladder with inflammable air, received immediately from the vessel containing the iron and diluted oil of vitriol, from which it was generated, I found that thirty ounce measures of it weighed more than seventeen grains less than an equal bulk of common air; but when I weighed that inflammable air of the same kind which had been confined by water in the same bladder, it was only fourteen grains less than an equal quantity of common air. This I repeated several times with the same result. This air, therefore, could only be about three times lighter than common air; whereas the other was more than ten times lighter.

Having frequently examined the specific gravity of inflammable air which has been long confined by water, by weighing it in a bladder, and then pressing out the air, and weighing it when empty (which has the same effect as weighing it full of common air) I have seldom found such air more than five times lighter than an equal portion of common air; so that the specific gravity of this air is soon doubled by being kept in these circumstances.

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SECTION IV.

Of Air produced by Substances putrefying in Water.

THE experiments recited in this and the following section were entered upon chiefly to discover the *principle of nutrition* in vegetable and animal substances; and they seem to lead us to suppose, that this principle is phlogiston, or the principle of inflammability, in such a state as to be capable of becoming, by putrefaction, a true inflammable air, but not generally such as to burn with explosions, but rather with a blue and lambent flame, mixed with a certain proportion of fixed air.

In the putrefactive process the phlogiston is merely evolved, and not again combined with any thing, except what may be necessary to its assuming the form of inflammable air; but in nutrition it is immediately held in solution by the gastric juice, and in the chyle formed by it. But if any part of the aliment pass the stomach, and the first intestines, without having all its phlogiston incorporated with the chyle, that principle remains in the excrement, where it is often set loose
in

in the form of inflammable air, the same form that it would have taken if it had gone through the simple putrefactive process. The phlogiston of the aliment, thus entering into the circulation with the chyle, after answering purposes in the animal œconomy which are yet very imperfectly known to us, is thrown out again by means of the blood in the lungs, and communicated to the air, which is phlogisticated by it.

All alimentary substances not only contain phlogiston, but I believe are capable of yielding a proper inflammable air by putrefaction. But in the following experiments on such vegetables as are generally used for food, *roots* seem to yield it in a greater abundance than other parts of plants; but there are some remarkable differences among them in this respect. For though potatoes are exceedingly favourable to the growth of that green vegetable substance, which yields pure air so copiously, owing probably to the phlogiston they contain, *onions*, perhaps equally nutritive with potatoes, are exceedingly unfriendly to that plant; but then they yield inflammable air in an astonishing quantity, when they are left to putrefy in water. This I rather suspect is a proof, that onions contain more phlogiston, and are the more nutritive substance, of the two.

On

On the 28th of June I exposed to the sun eighteen penny-weights of onions, in a jar of an hundred ounces of river water, inverted in a basin of the same. They presently began to yield air, but without ever becoming green; and on the 15th of July the quantity was fifteen ounce measures, a small part of which was fixed air, and the rest strongly inflammable. The water was white and turbid, and the air had a strong smell of onions.

About the same time I observed that it made no difference, with respect to the quality of this air, whether the onions were placed in the light or in the dark, the principle of vegetation not being concerned in this case. And though I observed the following differences in the quantities of air produced in the sun and in the shade, they were not uniform, and therefore must have depended upon some unknown accidental circumstances.

On the 17th of July I put two onions, each weighing an ounce and a quarter, in the sun, and two others of the same size, in a similar jar in the dark. On the 23d I examined them, and had twenty four ounce measures of air in the shade, and only twelve from those in the sun; but the latter was more strongly inflammable than the former, which burned with more of a lambent flame, though both exploded in some measure, so

as

as to be something more inflammable than air from marshes.

Having kept a quantity of this air, from the time above-mentioned to the 20th of July, 1780, I found it then strongly inflammable, little inferior to the inflammable air from metals. Perhaps the fixed air, which had been mixed with it before, was now completely expelled from it. It appears, however, that this kind of inflammable air has an inflammability of as permanent a nature as any whatever. The air from marshes also, which, with Sig. Volta, I doubt not comes from putrefying vegetable substances, I have also found to be equally permanent.

On the 1st of August I took two halves of the same onion (which was an old one, and beginning to sprout) each half weighing seventeen penny-weights twelve grains, and I placed one of them in the sun, and the other in the shade, both in similar receivers. On the 24th of the same month, that in the sun had given an ounce measure and three quarters of air, of which one fifth was fixed air, and the rest inflammable. From that in the dark I took two ounce measures and a quarter of air, one third of which was fixed, and the rest inflammable. From these experiments I was ready to conclude, that onions (and therefore, probably, other vegetable substances) would always give more

air in the dark than in the light; but the following experiments shewed that this is by no means the case always.

The 30th of July I placed in the sun, in a vessel containing fifty ounces of water, a part of a fresh gathered onion, weighing nine penny-weights, and also another part of the same onion, and of the same weight, in a vessel of the same size in the dark. On the 24th of August that in the sun had yielded three ounce measures of air, all inflammable, and that in the dark had produced as nearly as possible the same quantity, and as inflammable, when the fixed air that was mixed with it was washed out of it. The fixed air which had been extricated in the sun had been dissipated by means of the free access of fresh air.

Upon a former occasion I got only fixed air from onions confined by quicksilver; but then they wanted moisture, or were not kept till they were properly putrid. For I have since got inflammable air, as well as fixed air, from onions kept in quicksilver, from the 2d of September, 1779, to the 31st of March, 1780. The onions weighed twelve penny-weights twenty grains, and the air was half an ounce measure, three fourths of which was fixed air, and the rest inflammable. It appears from this, as well as from many other observations which I shall have occasion to mention hereafter, that
neither

neither fixed air, inflammable air, or nitrous air, can be produced without a considerable quantity of water, part of which we may therefore, with great probability, infer enters into the composition of these kinds of air; though when they *are* formed, we may not know any method of discovering, and re-producing that water.

Both *carrots* and *parsnips* yield great quantities of inflammable air, and equally in the sun or in the shade. I was at one time much amused with observing the inflammable air issuing from one of the carrots in the sun. It came sometimes in a constant stream, or in large successive bubbles, from one particular place, neither at the centre, nor near the outside of the carrot, but in the place where the air holes are the largest.

To ascertain the quantity of air produced from a given weight of these two roots, I placed as much of a parsnip as, by expelling water from a cylindrical vessel, I found to occupy the space of two ounce measures and a quarter of water, in the sun; and the next day I took from it four ounce measures of air, all fixed air, the residuum extinguishing a candle. This was on the 29th of July, and on the 31st of the same month I took from it four ounce measures more, of which two thirds of an ounce measure was inflammable. On the 2d of August I again took from it four ounce measures,

one fourth of which was inflammable, exploding with a blue flame. Lastly, on the 24th of August, perceiving that no more air would be produced, I took from it one third of an ounce measure; one third of which was fixed air, and the rest not inflammable, but phlogisticated.

From carrots occupying the space of an ounce measure and a half of water, exposed to the sun in rain water, from the 26th to the 31st of July, I took ten ounce measures of air, of which an ounce measure and half was strongly inflammable, exploding with a red flame; and on the 4th of August I took from them near four ounce measures of air, of which more than one half was inflammable. The water, which had a large surface, had probably absorbed much of the fixed air. This, however, was all the air that these carrots would yield.

An equal weight of carrots, exposed the same time in the dark, yielded nearly the same quantity of air, but only a small proportion of it was inflammable. This, however, I do not attribute to the darkness, but to some other unknown circumstance.

A sliced *turnip* fresh gathered, weighing near three ounces, exposed in the sun in rain water, yielded twelve ounce measures of air, one third of which was fixed air, and the rest strongly inflammable.

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On the 30th of July two ounces of turnip, fresh gathered, were placed in the dark, in a vessel containing seventy ounce measures of water; and on the 24th of August I took from it an ounce measure and a quarter of air, of which one ounce measure was phlogisticated, not inflammable. The water was exceedingly offensive. This phlogisticated air had been, I doubt not, inflammable in its origin, and in much greater quantity. When a turnip was sliced very thin, and the quantity of water large, I shall observe, that dephlogisticated air is produced.

Fruits, I found by no means favourable to the production of pure air. Like the preceding roots, they putrefied, and yielded inflammable air, mixed with fixed air. From *peaches*; both in the sun and in the shade, I got air, three fourths of which was fixed air, and the rest inflammable; but on this occasion the quantity of air produced in the sun was twice as much as that produced in the shade; though the quantity of water in which they were exposed was the same, and the peaches themselves were, as far as I could perceive, of the same size, and in the same state.

I placed two *Morella* cherries, one in the sun, and the other in the shade, in equal vessels of water. From that in the sun I got one third of

an ounce measure of air, and from that in the shade one fifth of an ounce measure, both inflammable. I had the same result with apricots.

Having found the capacity of these nutritive substances to yield inflammable air, I next tried whether they would part with any of it in *boiling*. But I found that none of them did, but only in *putrefying* afterwards; so that this mode of preparation (and the same I doubt not would be found to be the case with roasting, &c.) does not deprive any of these aliments of any part of their nutritive power.

From nineteen penny-weights eighteen grains of *onions* I expelled, by boiling in river water, half an ounce measure of air, of which one third was not absorbed by water, and extinguished a candle.

From one ounce fifteen penny-weights of *lettuce* I got three quarters of an ounce measure of air, of which half an ounce measure was phlogisticated air.

From one ounce sixteen penny-weights twelve grains of *carrots* I got three quarters of an ounce measure of air, of which about one ounce measure was phlogisticated air.

These differences are inconsiderable, and some of the air, no doubt, came from the water in which

which these substances were boiled. Afterwards the potatoes and carrots, putrefying in water, yielded each more than two ounce measures of air, one half of which was fixed air, and the rest inflammable. The onions yielded only about half an ounce measure of air, but it was of the same kind, and the lettuce gave only a tenth of an ounce measure, in which nothing could be perceived to be inflammable. But I did not begin to collect this air till a day or two after the process of boiling, when I perceived some of the substances to be in a state of yielding air.

SECTION V.

Of Air produced by various Substances putrefying in Quicksilver.

IN some of the first of my experiments I amused myself with putting different vegetable and animal substances into tall glass vessels, previously filled with mercury, and the following were among the results which I then noted.

If beef or mutton, raw or boiled, be placed so near to the fire, that the heat to which it is exposed shall equal, or rather exceed, that of the blood, a considerable quantity of air will be generated in a day or two, about one seventh of which I have generally found to be absorbed by water, while all the rest was inflammable: but air generated from vegetables, in the same circumstances, will be almost all fixed air, and no part of it inflammable. This I have repeated again and again, the whole process being in quicksilver; so that neither common air, nor water, had any access to the substance on which the experiment was made; and the generation of air, or effluvia of any kind, except what might be absorbed by quicksilver, or resorbed by the substance itself, might be distinctly noted.

A veget-

A vegetable substance, after standing a day or two in these circumstances, will yield nearly all the air that can be extracted from it, in that degree of heat; whereas an animal substance will continue to give more air or effluvium, of some kind or other, with very little alteration, for many weeks. It is remarkable, however, that though a piece of beef or mutton, plunged in quicksilver, and kept in this degree of heat, yield air, the bulk of which is inflammable, and contracts no putrid smell (at least, in a day or two) a mouse treated in the same manner, yields the proper putrid effluvium, as indeed the smell sufficiently indicates.

By means of these experiments, and those in the preceding section, it may be possible to determine the nutritive powers of different vegetable and animal substances, and also other problems in philosophy; though too much must not be expected from them.

It might have been imagined, that by this means we should be able to ascertain the quantity of air that any mass of putrescent matter would thoroughly phlogistificate. For any given quantity of inflammable air will completely phlogistificate twice its bulk of common air. But it will be found that a putrefying mouse will phlogistificate much more than that proportion of air. There must, therefore, be much more phlogiston in a mouse than
forms

forms the inflammable air which comes from it. Perhaps, therefore, that phlogiston which contributes to animal nutrition, may also be more than that which enters into the composition of the inflammable air that comes from the putrefying substance. This is a subject that requires and deserves much farther investigation. I only recite the following as *leading experiments*, to the solution of greater problems. They are, indeed, upon too small a scale to be of much use even for this purpose; except to shew that the same kind of substance, which in a large quantity yields inflammable air, in a small quantity may yield phlogisticated air.

A small *fish*, weighing forty four grains, being confined in quicksilver from the 21st of May to the 24th of August, gave something more than half an ounce measure of air, two thirds of which was fixed air, and the remainder extinguished a candle, but was not sensibly inflammable.

From two pennyweights of well boiled *beef* I got a very small quantity of air,* the bulk of which was fixed air, and the rest not inflammable. At another time, from one pennyweight and nineteen grains of *raw beef*, I got 0.22 of an ounce measure of air, nine tenths of which was fixed air, and the rest extinguished a candle.

From fifty-three grains of *raw lamb*, I got 0.17 of an ounce measure of air, the bulk of which was
fixed

fixed air, and the rest not sensibly inflammable: but from two pennyweights and two grains of well *roasted lamb*, I got three quarters of an ounce measure of air, half of which was fixed air, and the rest highly inflammable; and some time after I took from the same substance half an ounce measure of air more, of which three fourths was fixed air, and the rest inflammable.

From thirteen pennyweights and four grains of the *tendon* of a roasted neck of veal, I got an ounce measure and half of air, of which half was fixed air, and the rest phlogisticated. Afterwards I took from it one ounce measure and three quarters of pure fixed air, with the smallest residuum possible. In the former experiment also, as well as on a former occasion, I found that the inflammable air was extricated first, and a long time before all the fixed air was exhausted.

Having had occasion to make many experiments with putrefying *mice*, and having more in prospect, I was particularly desirous to ascertain the quantity and quality of the air produced by a mouse of the middle size putrefying in quicksilver, and I found as follows. A mouse weighing six pennyweights and three grains, confined by quicksilver, which had putrefied from the 8th of April, had yielded on the 24th of July one ounce measure and three quarters of

of air, of which one fourth was weakly inflammable, and the rest fixed air. This I found, by other experiments, was nearly as much as a mouse would yield in these circumstances.

Having left another mouse to putrefy in quicksilver, I took the air produced from it at different times, in order to satisfy myself more fully with respect to the proportion that the fixed and inflammable air bore to each other, from the beginning to the end of the process. The mouse weighed five pennyweights and ten grains, and it was put into an inverted vessel of quicksilver on the 13th of June. On the 26th of that month, I took from it near an ounce measure of air, three fourths of which was fixed air, and the rest inflammable, burning with a very blue flame. On the 16th of August I took from it an ounce measure and a quarter of air, of which four fifths was fixed air, and the rest, if it was inflammable at all, was so in the slightest degree imaginable; and lastly, on the 3d of April following, I took from it a small quantity of air, perhaps one tenth of an ounce measure, the whole of which was, as far as I could judge, all fixed air.

When a mouse is left to putrefy in this manner, there comes from it a great quantity of dissolved blood, or some other thin reddish liquor. This I carefully separated from what was *solid* in the mouse, and

and found that this continued to give air, when the liquor gave little or none; so that perhaps it may be something *solid* in all bodies that contributes to the formation of permanent air. By long standing, however, I did get a little air from this red liquor, and it was almost all fixed air. It was, perhaps, combined with it, at its separation from the mouse.

The experiments on some of the different *parts* and *secretions* of animal bodies were made on the same small scale with most of the preceding, and therefore they can only have the same imperfect use.

From seven pennyweights of the medullary part of a sheep's *brain* raw, I got four and a half ounce measures of air, of which one fifth part of an ounce measure was inflammable, and the rest fixed air. I also found by similar experiments, that the cortical part of the same brain gave somewhat less air than the medullary part; but the proportion of the inflammable to the fixed air was the same. No certain inference, however, can be drawn from experiments on so small a scale as these.

Two pennyweights of *mutton* *gravy* yielded 0.02 of an ounce measure of air, the greatest part of which was fixed air, and the remainder seemingly inflammable.

Two pennyweights of the *crassamentum* of sheep's blood gave only a small bubble of air, too small to be examined. The *serum* also yielded some air, the
bulk

bulk of which was fixed air, and the rest phlogisticated.

An ounce measure of *milk* yielded near half an ounce measure of air, almost pure fixed air, a small remainder being phlogisticated.

An ounce measure and an half of the *bile* of a sheep yielded half an ounce measure of air, almost all fixed air, the small residuum being phlogisticated.

I should not have made these experiments on so very small a scale, but that I expected a greater quantity of air from all the substances, and because less quicksilver was wanted for the purpose; so that I could have more processes going on at the same time. Had the same substances putrefied in *water*, they would have yielded many times more air, water appearing to be an essential ingredient in the constitution of inflammable air.

PART

P A R T II.

OF THE PROPERTIES OF INFLAMMABLE AIR.

SECTION I.

Various Experiments to change and decompose inflammable Air.

1. *Inflammable Air diminished by Charcoal.*

IN pursuance of the Abbé Fontana's experiment on the absorption of air by charcoal, I dipped pieces of hot charcoal into a phial of inflammable air, and immediately inverted it in quicksilver. When one third of the whole quantity was imbibed, I found that both the remainder, and that which was again expelled from the charcoal, by plunging it in water, was inflammable; the former not to be distinguished from what it had been, but the latter a little less inflammable.

Of

2. *Of Putrefaction in inflammable Air.*

Though air tainted with putrefaction extinguishes flame, I have not found that animals or vegetables putrefying in inflammable air render it less inflammable. But one quantity of inflammable air, which I had set by in May, 1771, along with the others above-mentioned, had had some putrid flesh in it; and this air had lost its inflammability, when it was examined at the same time with the other in the December following. The bottle in which this air had been kept, smelled exactly like very strong Harrogate water. I do not think that any person could have distinguished them.

3. *Plants growing in inflammable Air.*

I have made plants grow for several months in inflammable air made from zinc, and also from oak; but, though they grew pretty well, the air still continued inflammable. The former, indeed, was not so highly inflammable as when it was fresh made, but the latter was quite as much so; and the diminution of inflammability in the former case, I attribute to some other cause than the growth of the plant.

Water

4. *Water impregnated with inflammable Air.*

Neither does inflammable air undergo any change by impregnation with water, in which respect, it agrees with what I have observed of nitrous air. For having impregnated a quantity of rain water (out of which all its air had been carefully extracted by the air pump) with inflammable air, of which it imbibed about one thirteenth of its bulk; about a month afterwards, by making it boil in a phial, I expelled from it about the same quantity of air, and found it to be as strongly inflammable as it had ever been. After this process there was a deposit from the water of a filmy kind of matter, probably the earth of the metal that had been employed in producing the inflammable air. In both these respects inflammable air resembles nitrous air.

Having had the curiosity, on the 25th of July, 1772, to expose a great variety of different kinds of air to water out of which the air it contained had been boiled, without any particular view; the result was, in several respects, altogether unexpected, and led to a variety of new observations on the properties and affinities of several kinds of air with respect to water. Among the rest three fourths of that which was inflammable was ab-

forbed by the water in about two days, and the remainder was inflammable, but weakly so.

Upon this, I began to agitate a quantity of strong inflammable air in a glass jar, standing in a pretty large trough of water, the surface of which was exposed to the common air, and I found that when I had continued the operation about ten minutes, near one fourth of the quantity of air had disappeared; and finding that the remainder made an effervescence with nitrous air, I concluded that it must have become fit for respiration, whereas this kind of air is, at the first, as noxious as any other kind whatever. To ascertain this, I put a mouse into a vessel containing two ounce measures and a half of it, and observed that it lived in it twenty minutes, which is as long as a mouse will generally live in the same quantity of common air. This mouse was even taken out alive, and recovered very well. Still also the air in which it had breathed so long was inflammable, though very weakly so. I have even found it to be so when a mouse has actually died in it. Inflammable air thus diminished by agitation in water, makes but one explosion on the approach of a candle, exactly like a mixture of inflammable air with common air.

From this experiment I concluded that, by continuing the same process, I should deprive inflammable

mable air of all its inflammability, and this I found to be the case; for, after a longer agitation, it admitted a candle to burn in it, like common air, only more faintly; and indeed by the test of nitrous air it did not appear to be near so good as common air. Continuing the same process still farther, the air which had been most strongly inflammable a little before, came to extinguish a candle, exactly like air in which a candle had burned out, nor could they be distinguished by the test of nitrous air.

I took some pains to ascertain the quantity of diminution, in fresh made and very highly inflammable air from iron, at which it ceased to be inflammable, and, upon the whole, I concluded that it was so when it was diminished a little more than one half: for a quantity which was diminished exactly one half had something inflammable in it, but in the slightest degree imaginable. It is not improbable, however, but there may be great differences in the result of this experiment.

This change in the inflammable air proceeded, I doubt not, from its communication with the external air through the water; so that I should not expect the same change from the agitation of it in close vessels. Phlogisticated air is meliorated by agitation in open vessels, but not in close ones.

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Finding

Finding that water would imbibe inflammable air, I endeavoured to impregnate water with it, by the same process by which I had made water imbibe fixed air; but though I found that distilled water would imbibe about one fourteenth of its bulk of inflammable air, I could not perceive that the taste of it was sensibly altered.

5. *Inflammable Air agitated in Oil of Turpentine.*

The effect of agitating inflammable air in oil of turpentine, and also in spirit of wine, is not a little remarkable. They seem to bring it at last to the same state to which it is brought by agitation in *water*, only that, whereas it is *diminished* by the process in water, it is *increased* in these processes. Both these substances, however, as well as water, seem to deprive this air of part of its phlogiston. The facts, as I observed them, were as follows.

Having agitated a quantity of inflammable air in oil of turpentine, I presently observed an increase of its quantity, and I continued the process till it had increased one half. Agitation in spirit of wine produced the same effect, but more time was requisite for it. Allowing it to continue in these circumstances all night, I found that one half of the additional quantity of air had disappeared;
but

but by repeating the agitation about a quarter of an hour, it was again increased as much as before. I then examined it, and found that it was not in the least absorbed by water, did not affect lime water, was but very slightly inflammable, and was diminished by nitrous air almost as much as common air; which is in all respects the very state to which agitation in water would have brought it, except that in water it would have been considerably diminished, instead of being increased.

I agitated another quantity of inflammable air in oil of turpentine made pretty *warm*, but the effect was the very same as when it was cold. In this case, however, though I hardly ever discontinued the agitation, after I had begun it, when it had gained an increase of about one fourth of its bulk, it lost it again; and was reduced to its original dimensions. I then examined it, and found it to burn with a lambent blue flame. I own myself to be intirely at a loss to account for the increase and decrease of the quantity of air in these experiments.

6. *Animals dying in inflammable Air.*

Inflammable air kills animals as suddenly as fixed air, and, as far as can be perceived, in the same manner, throwing them into convulsions,

and thereby occasioning present death. I had imagined that, by animals dying in a quantity of inflammable air, it would in time become less noxious; but this did not appear to be the case; for I killed a great number of mice in a small quantity of this air, which I kept several months for this purpose, without its being at all sensibly mended; the last, as well as the first mouse, dying the moment it was put into it.

7. *Inflammable Air changed by keeping in Water.*

Inflammable air is not thought to be miscible with water, and when kept many months, seems, in general, to be as inflammable as ever. Indeed, when it is extracted from vegetable or animal substances, a part of it will be imbibed by the water in which it stands; but it may be presumed, that in this case, there was a mixture of fixed air extracted from the substance along with it. I have indisputable evidence, however, that inflammable air, standing long in water, has actually lost all its inflammability, and even come to extinguish flame much more than that air in which candles have burned out. After this change it appears to be greatly diminished in quantity, and it still continues to kill animals the moment they are put into it.

This

This very remarkable fact first occurred to my observation on the 25th of May, 1771, when I was examining a quantity of inflammable air, which had been made from zinc, near three years before. Upon this, I immediately set by a common quart bottle filled with inflammable air from iron, and another equal quantity from zinc; and examining them in the beginning of December following, that from the iron was reduced near one half in quantity, if I be not greatly mistaken; for I found the bottle half full of water, and I am pretty clear that it was full of air when it was set by. That which had been produced from zinc was not altered, and filled the bottle as at first.

I think that, in all, I have had four instances of inflammable air losing its inflammability, while it stood in water. It is very possible, however, that there might be some impregnation in this water, of which I was not aware, since other persons, I find, have not found any change in inflammable air by keeping it in pure water.

November 6, 1772, a quantity of inflammable air, which, by long keeping, had come to extinguish flame, I observed to smell very much like common air in which a mixture of iron filings and brimstone had stood. It was not, however, quite so strong, but it was equally noxious,

8. *The electric Spark in inflammable Air.*

No kind of air, on which I have yet made the experiment, will conduct electricity; but the colour of an electric spark is remarkably different in some kinds of air, which seems to shew that they are not equally good non-conductors. In fixed air, the electric spark is exceedingly white; but in inflammable air it is of a purple, or red colour. Now, since the most vigorous sparks are always the whitest, and, in other cases, when the spark is red, there is reason to think that the electric matter passes with difficulty, and with less rapidity: it is possible that the inflammable air may contain particles which conduct electricity, though very imperfectly; and that the whiteness of the spark in the fixed air, may be owing to its meeting with no conducting particles at all. When an explosion was made in a quantity of inflammable air, it was a little white in the center, but the edges of it were still tinged with a beautiful purple. The degree of whiteness in this case was probably owing to the electric matter rushing with more violence in an explosion than in a common spark.

9. *The Smell of inflammable Air.*

Inflammable air, when it is made by a quick process, has a very strong and offensive smell,
from

from whatever substance it be generated, but this smell is of three different kinds, according as the air is extracted from mineral, vegetable, or animal substances. The last is exceedingly fetid; and it makes no difference, whether it be extracted from a bone, or even an old and dry tooth, from soft muscular flesh, or any other part of the animal. The burning of any substance occasions the same smell: for the gross fume which arises from them, before they flame, is the inflammable air they contain, which is expelled by heat, and then readily ignited. The smell of inflammable air is the very same, as far as I am able to perceive, from whatever substance of the same kingdom it be extracted. Thus it makes no difference whether it be got from iron, zinc, or tin, from any kind of wood, or, as was observed before, from any part of an animal.

If a quantity of inflammable air be contained in a glass vessel standing in water, and have been generated very fast, it will smell even through the water, and this water will also soon become covered with a thin film, assuming all the different colours. If the inflammable air have been generated from iron, this matter will appear to be a red ochre, or the earth of iron, as I have found by collecting a considerable quantity of it; and if it have

have been generated from zinc, it is a whitish substance, which I suppose to be the calx of the metal. It likewise settles to the bottom of the vessel, and when the water is stirred, it has very much the appearance of wool. When water is once impregnated in this manner, it will continue to yield this scum for a considerable time after the air is removed from it. This I have often observed with respect to iron,

SECTION II.

Inflammable Air decomposed by Heat, in Tubes of Flint Glass.

THIS kind of air remains unchanged when it is exposed to heat in a tall jar of flint glass, in which it had free liberty to expand. I made this experiment at the same time with the similar one that I shall have occasion to mention on nitrous air. This air, as well as the nitrous, recovered its former dimensions when it was cold, and appeared to be unchanged in its quality.

A very

A very singular decomposition of inflammable air I observed in consequence of exposing a great variety of substances to the influence of a sand heat, which I kept up for several months. Among other things, I buried in this hot sand glass tubes hermetically sealed, and previously filled with all the different kinds of air. I filled them in the following manner.

Having provided myself with glass tubes about four feet long, and about one third or one half of an inch in diameter, and of such a thickness as that I could easily melt them with the flame of a couple of candles and a common blow pipe, I first sealed the tubes at one end, then filled them with quicksilver, and placed them inverted in a basin of the same. After this, either transferring the air in a bladder, from the jars in which they had been standing in water, or generating the air afresh, if it was of a kind not to bear the contact of water, I filled the tubes completely with the kinds of air on which I wished to make the experiment, displacing the quicksilver. This being done, I inclined the tube, and applying the flame of my candles with some care (holding the blow pipe in my mouth only, and keeping firm hold of the tube on each side of the place to which I was applying the heat) I melted the glass, and took off what lengths of it I pleased; and
every

every piece was, of course, hermetically sealed. These pieces I marked with a file, keeping an account of the meaning of the marks, that when I took them out of the sand, I might presently know with what kind of air they had been filled.

When I was performing this part of the process with inflammable air in flint glass tubes, I observed that the places to which I applied the heat were generally tinged black; but I gave little attention to this circumstance, thinking it might be something accidental; and without any particular expectation, I buried these tubes in the sand, together with the others. This was on the 25th of September, 1777.

On the 20th of January following, I examined these tubes, together with every thing else that had been exposed to the same heat. The tube containing the inflammable air was ten inches long, and by some accident was broken; but it was jet black throughout. At this I was very much surprized, but I did not then suspect that it was at all owing to the inflammable air with which it had been filled; thinking it might have been occasioned by some phlogistic matter in the sand, or in some of the vessels that had burst in its neighbourhood.

Reflecting, however, on this odd circumstance, and thinking, from the uniformity of the tinge, that,

that, *possibly*, it might have been occasioned by the inflammable air, I filled another small glass tube with the same air; and, sealing it hermetically, buried it deep in sand, contained in an iron pot, which I set on the fire, and made very hot, nearly red; and taking it out the next day, I found the tube quite black, except a small part on one side of that end which had been uppermost, about two inches higher than the other, and which, consequently, had not been exposed to so great a degree of heat.

Being now fully satisfied that the blackness of the tube was *certainly* occasioned by heating the inflammable air within it, in circumstances in which it could not expand, I proceeded to examine the state of the air, and frequently found it to be inflammable; but, in general, the quantity was too small to make a satisfactory experiment.

Putting two glass tubes, about four inches in length, and a quarter of an inch in diameter, into a sand furnace, I kept them in it two days; when I took them out, and observed that the tube which I had placed at the bottom of the sand, in the greatest degree of heat, was nearly melted, and perfectly *blue*, like indigo; while the other tube, which had not been exposed to so great a degree of heat, was of a beautiful jet black throughout.

At one time I had a suspicion that this blackness communicated to the glass was something precipitated

tated from the iron, by the solution of which the inflammable air had been made; but I was soon convinced of the contrary, by finding that the effect was the very same when the inflammable air was made from *zinc*.

I soon found that there was no occasion for so long a process to produce this effect, at least upon the glass. For it began to be discoloured the moment it was red hot, or rather when it became soft; as was evident by holding one of the tubes in an open fire, or in the flame of a candle. For wherever the heat was applied, the blackness immediately took place, without affecting any other part of the tube.

When I examined this black tinge narrowly, I found that it did not penetrate the glass, but formed a delicate superficial tinge, leaving the glass as perfectly polished as before the process. But the blackness was indelible; at least, it could not be scraped off without tearing the surface of the glass, and it made no change in it with respect to electricity. For the tube thus blackened was as perfect a non-conductor as ever.

The blue colour of the glass that was most heated, Mr. Delaval informed me, was owing to something of *iron* in the composition of the glass. That it also depended upon the *degree of heat*, I ascertained by placing one of these tubes in a vertical position in the

sand heat. For the lower end of the tube, which was most heated, had acquired a deep blue colour, and it passed into the black at the upper end of the tube without any intermediate colour. There was also no other colour higher than the black; so that the first tinge that the glass receives is a perfect black. Yet viewing the first tinge that it receives by the light of a candle placed beyond it, it seemed to have a shade of *red*.

As I was sensible that the blackness was owing to the precipitation of *phlogiston* from the inflammable air, I thought it possible that some substance which had a near affinity with phlogiston might discharge it; and trying *minium*, it succeeded immediately. Having filled one of these black tubes with this metallic calx, the moment I made it red hot, the blackness intirely disappeared, and left the tube as transparent as ever it had been.

In the first experiment of this kind I used minium, out of which all its air had been expelled by heat, and which is of a yellow colour. In this process it became whiter, and adhered a little to the glass. When I scraped it off, I could not be quite sure that any part of it was become real *lead*; but it evidently approached towards a metallic state, by being of a more compact texture than before.

In this state of the experiments I communicated the result of my observations to my friend Mr. Bewly, who suggested to me, that, probably, it was
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the *lead* in the glass tubes that had attracted the phlogiston; and I presently found this to be the case. For when I had filled a *green glass* tube with the inflammable air, and sealed it hermetically, as I had done the flint glass tubes, I exposed it to a melting heat, which is greater than that which flint glass will bear, without producing any change of colour in it. What remained of the air in the tube, that did not escape when part of it was melted, was still strongly inflammable.

It appears, therefore, from this experiment, that the calx of lead, in the form of glass, has a stronger affinity with phlogiston than any thing in the composition of inflammable air, in a degree of heat capable of melting glass. Or, if there be no proper constituent part of inflammable air besides phlogiston, the attraction of the calx is so great, as to reduce the phlogiston from an elastic and uncombined state to a fixed and combined one.

Having, by means of these glass tubes, effected a complete decomposition of inflammable air, the phlogiston in it having united with the glass of the lead; I thought that, if there had been any *acid* in its composition, it would then be disengaged, and be found in the tube. In order to find whether there was any acid in it, or not, I poured into one of these tubes a small quantity of water made blue with the juice of turnsole; but it came out as blue as it went in.

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SECTION III.

Of sulphurated inflammable Air.

THERE is no kind of air which admits such a variety of modifications as the inflammable; nor shall we think this extraordinary, when we consider that phlogiston, which is the distinguishing ingredient in it, enters into a greater variety of combinations with *solid substances* than perhaps any other principle in nature, and is the cause of a greater variety of properties in them. Spirit of wine, oil, sulphur, charcoal, and metals, are substances as different from each other, both in their external appearance, their degrees of consistence, and other chemical properties, as any things in nature, and yet the principal ingredient in them all is the same phlogiston; as may be proved by the actual transferring of it from any one to any other of them. Inflammable air likewise extracted from each of these substances; as also that from putrid vegetables, and by other processes, of which an account has been given in the preceding sections, are all remarkably different, and appear to be so, as we shall presently see, when they are decomposed. I shall now give an account

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of another species of this kind of air, which I term *sulphurated*, from the strong smell that it has of sulphur; or rather liver of sulphur, and its being loaded with a greater quantity of matter; which, though at the first black, yet on exposure to the air presently assumes a yellowish colour. I shall recite the experiments in which I observed this peculiar species of air, in the order in which I made them, noting the other appearances that accompanied them, though they have not any immediate relation to the *air* of which I am treating.

When I was engaged in that course of experiments in which steam, and the vapour of various fluid substances, was brought into contact with solid substances red hot, I treated *manganese* in this manner, and especially a quantity with which Mr. Woulfe had formerly furnished me, which was not in powder, but in a large mass, just as it is dug out of the earth. A few ounces of this I put into an earthen tube, open at both ends. But closing one of them with a cork, while the middle part of the tube was red hot, and the other orifice was furnished with an apparatus proper for collecting the air that might be expelled from it, I received forty ounce measures of air, of which one sixth was fixed air, and the rest of the standard of 1.7, lambently inflammable. No more air coming in this disposition of the apparatus, I opened

I opened the other end of the tube, and with a proper contrivance for the purpose, sent through it a quantity of steam; in which circumstances air was produced more copiously than before. Of this I received about fifty ounce measures, observing that one seventh of it was fixed air, and the rest of the standard of 1.8, not lambently, but explosively, inflammable. The last portions of this air were very turbid, and the smell of the air, and especially that of the last portion, was very sulphureous, and, I observed, tinged the water of a very dark colour, by depositing in it a quantity of blackish matter. However, the air itself became presently transparent, and had no other appearance than that of any other kind of air, when I left in my trough a jar filled with it.

Having been intent on some other experiments, I was surprised to find, on looking on the jar about ten minutes afterwards, that it was quite black, so that I could see nothing in the inside of it. In order to observe how it came to be so, I afterwards filled another jar with this kind of air, and observed that when the water was well subsided, black specks began to appear in different places, and, extending themselves in all directions, at length joined each other, till the whole jar was perfectly black, and the glass quite opaque. When this was done, I transferred the air to another

clean jar, and it soon produced the same effect upon this, though it never became so black as the jar in which it had been first received. It also frequently happened that only the lower part of the jar would become black, as if this matter, with which it was loaded, had kept subsiding, though invisibly, in the mass of air, and occupied the lower regions of it only, leaving the upper part entirely free from it. When the vessels thus tinged black were exposed to the open air, that colour presently disappeared, and a yellow or brown incrustation was left upon it.

Thinking, from this circumstance, that this black coating consisted of some volatile phlogistic matter, I placed the jars which had this black tinge with their mouths inverted in vessels of water, in order to observe the effect which the change of colour might have on the common air contained in them. In these circumstances the black tinge presently went off, and was succeeded by the yellow colour, but without producing any sensible change in the air. In some cases, however, I thought that it was injured; but it was by no means so much so as I had expected. After depositing this black matter, the air still retained its sulphureous smell, and as far as I can judge, will never entirely leave it.

It is by no means the universal property of manganese to yield this sulphurated inflammable air, but must have been owing to something peculiar to this specimen, and perhaps to something accidentally mixed with it. For when I repeated the experiment with other manganese, which I had from a glass house, in which it is used, I had no such appearance. From four ounces of this manganese, treated as the preceding, I got without steam, 256 ounce measures of air, of which about one tenth was fixed air. Then sending steam over it, I got more air, but in no great quantity, about ten ounce measures in an hour; though probably much more might have been procured, if the process had been continued. This was dephlogisticated; for, mixed with two equal measures of nitrous air, the standard was 0.28, which shews that it was exceedingly pure. But one tenth of this was fixed air, as in the former portion, which agrees with the experiments I formerly made with this substance when I found that heat alone would expel from it a quantity of very pure air.

The next time that I got this sulphurated inflammable air, was as unexpected as the preceding; and this experiment was the first thing that gave me any insight into the nature of it. Having occasion to make a large quantity of inflammable air, instead of fresh turnings of iron, I happened

to take some, parts of which had been heated by a burning lens in vitriolic acid air, in which, as I have observed, it melts with great readiness, and gathers into balls. When this iron was dissolved in diluted oil of vitriol, though there were only a few pieces in the quantity that I used which had been melted in this manner, the water in which the air was received was very black, and deposited more sediment than in the experiment with the manganese. The jars also which contained it were presently as black as ink, but became yellow when exposed to the open air. This inflammable air had also the same offensive sulphureous smell; so that there could be no doubt of its being the same kind of air which I had got from Mr. Woulfe's manganese. There was in it, however, a mixture of vitriolic acid air, as I perceived when I burned a large quantity of it in a glass balloon, in order to collect the *water* that might be produced in this process. All the inside of the balloon was filled with a dense white cloud, all the time that the air was burning in it, and the water produced was very sensibly acid. In reality, the same effects were produced as if sulphur had been burned in the vessel.

As I had no doubt, but that the iron which had been melted in vitriolic acid air was the same as what is called *sulphurated iron*, or iron with which
sulphur

fulphur is incorporated, I now completely ascertained it by making a quantity of fulphurated iron, dipping it when red hot into melted sulphur. This iron, treated as the other had been, yielded exactly such air as I have been describing, so that I could have no doubt with respect to the real origin of it.

When I decomposed this air, by firing it with an equal quantity of dephlogisticated air, the diminution of bulk was the same as when I used the common inflammable air, so that it did not appear to contain either more or less phlogiston; but there was a small quantity of *fixed air* produced, which is never the case with inflammable air procured with oil of vitriol, though it is sometimes when it is procured from iron with spirit of salt.

When the sulphurated inflammable air is received in vessels containing mercury, there is very little black matter deposited from it; but it appears when it is transferred into vessels containing water.

Though jars thinly coated with this black matter become yellow when exposed to the open air, this is not the case with that which is collected from the water in which the air has been confined. For when the water is evaporated from it, it adheres to the evaporating vessel in the form of a perfectly black incrustation. This substance, though

it does not burn blue on a hot iron, yet shews evident signs of containing sulphur. For when the nitrous acid has taken from it its superfluous phlogiston, it has both the colour and the smell of sulphur.

SECTION IV.

Metals, and other Substances containing Phlogiston, formed by imbibing inflammable Air.

THERE are few subjects, perhaps none, that have occasioned more perplexity to chemists, than that of *phlogiston*, or, as it is sometimes called, *the principle of inflammability*. It was the great discovery of Stahl, that this principle, whatever it be, is transferable from one substance to another, how different soever in their other properties, such as sulphur, wood, and all the metals, and therefore is the same thing in them all. But what has given an air of mystery to this subject, has been that it was imagined, that this principle, or substance, could not be exhibited except in combination with other substances, and could not be made

made to assume separately either a fluid or solid form. It was also asserted by some, that phlogiston was so far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they chose to call it *the principle of levity*. This opinion had great patrons.

Of late it has been the opinion of many celebrated chemists, Mr. Lavoisier among others, that the whole doctrine of phlogiston is founded on mistake, and that in all cases in which it was thought that bodies parted with the principle of phlogiston, they in fact lost nothing, but on the contrary acquired something; and in most cases an addition of some kind of air; that a *metal*, for instance, was not a combination of two things, viz. an *earth* and *phlogiston*, but was probably a simple substance in its metallic state; and that the calx is produced not by the loss of phlogiston, or of any thing else, but by the acquisition of air.

The arguments in favour of this opinion, especially those which are drawn from the experiments that Mr. Lavoisier made on mercury, are so specious, that I own I was myself much inclined to adopt it. My friend Mr. Kirwan, indeed, always held that phlogiston was the same thing with inflammable air. I did not, however, accede

to it till I thought I had discovered it by direct experiments, made with general and indeterminate views, in order to ascertain something concerning a subject which had given myself and others so much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red hot in flint glass tubes, gave them a black tinge, and was in a great measure absorbed, which I discovered to be owing to the calx of lead in the glass, attracting phlogiston from the inflammable air.

I found, however, great difficulty in repeating these experiments; and the quantity of inflammable air operated upon in them, is necessarily so small, that the result is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glass, surrounded with inflammable air, or rather on the calx of lead alone, in the same circumstances, would be a much easier experiment, and might bring me nearer to my object; and on making the experiment it immediately answered far beyond my expectation.

For this purpose, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient stand,
intro-

introduced it into a large receiver, filled with inflammable air, confined by water. As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black; and then ran in the form of perfect lead, at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell, except by actual trial, whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as, if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it, and something else. However, I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not, and if it had, what that base was. For seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt, but that the calx was actually imbibing something from the air; and from its effects in making the calx into metal, it could be no other than that to which

which chemists had unanimously given the name of *phlogiston*.

Before this first experiment was concluded, I perceived, that if the phlogiston in inflammable air had any base, it must be very inconsiderable: for the process went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be distinguished from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was, therefore, pretty well satisfied that this inflammable air could not contain any thing besides phlogiston; for at that time I reduced about forty five ounce measures of the air to five.

In order to ascertain a fact of such importance with the greatest care, I afterwards carefully expelled from a quantity of minium all the phlogiston, and every thing else that could have assumed the form of air, by giving it a red heat when mixed with spirit of nitre; and immediately using it in the manner mentioned above, I reduced a hundred and one ounce measures of inflammable air to *two*. To judge of its degree of inflammability, I presented the flame of a small candle to the mouth of a phial filled with it, and observed, that it made thirteen separate explosions, though weak ones (stopping the mouth of the

the phial with my finger after each explosion) when fresh made inflammable air, in the same circumstances, made only fourteen explosions, though stronger ones.

After this experiment I could not hesitate to conclude*, that this inflammable air went totally, and without decomposition, into the lead which I formed at that time; and if the necessary circumstances of the experiment be considered, it will be thought extraordinary that, even admitting this, the result should be so decisively clear in favour of it: for, in the first place, the greatest care must be used to expel all air from the minium, and it must be used before it can have attracted any from the atmosphere; and in the next place, the water also (a considerable quantity of which must be used, and which will also be heated in the process) should be made as free from air as possible. In these circumstances, had I found the small residuum, of two ounce measures from a hundred and one, to have been phlogisticated or fixed air, I should not have been disappointed; and it would not have prevented my conclud-

* In this conclusion, I overlooked one obvious consideration, viz. that water, or any thing soluble in water, might be the basis of inflammable air. All that could be absolutely inferred from the experiment was, that this basis could not be any thing that was capable of subsisting in the form of *air*. It will be seen, that I afterwards made the experiment with the air confined by mercury.

ing that *phlogiston* was the same thing with *inflammable-air*, contained in a combined state in metals, just as fixed air is contained in chalk and other calcareous substances; both being equally capable of being expelled again in the form of air.

Afterwards, using a calx of lead, which had been prepared in the same manner with the former, but which had remained for some weeks exposed to the air, I found, that when by using it I had reduced 150 ounce measures of inflammable air to ten, this residuum was phlogisticated air. But examining this calx separately, I found that it gave, by heat in a glass vessel, a considerable quantity of phlogisticated air.

I must observe, that the minium should not be reduced to a perfectly compact *glass of lead*; for then it will be too refractory to be easily revived by this process. Making use of some of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inside of the receiver: an experiment which I shall repeat and reconsider. I must also observe, that the lead which I procured in the above mentioned process was not to be distinguished from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made use of inflammable air from wood, I found, that though I was able to reduce minium with

with it, it was effected with more time and difficulty. Forty ounce measures of this kind of inflammable air I reduced to twenty five; after which I found that the heat of the lens produced only *glass of lead*, and no *metal*. The air was still, however, inflammable; and there was a small mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have some reason to think, consists of an intimate union of fixed air with that which is of the *explosive* kind extracted from metals. The result of those experiments which I made with that kind of inflammable air which is collected in the process for making phosphorus, and which burns with a lambent yellow flame, was similar to those which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable result with inflammable air, I immediately tried all the other kinds of air in the same manner; but in none of them did I procure any thing from the minium, besides glass of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogificated air, marine acid air, fluor acid air, as also in common and dephlogificated air, I got no *metal* at all. In vitriolic acid air there was but a small quantity of lead produced, and I have observed that this kind of air imparts a certain portion of phlogiston to common air (or rather imbibes

bibes a part of the dephlogisticated air from it) rendering the remainder in some measure phlogisticated, though by no means in so great a degree as nitrous air.

Though nitrous air and phlogisticated air certainly contain phlogiston, they appear by these experiments to hold it too obstinately to part with it to minium in this process, notwithstanding nitrous air quits it so readily to respirable air. I would observe, that there were some peculiar appearances in the experiments I made to revive the calx of lead in these kinds of air in which the attempt did not succeed; but I must repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead seems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more so; and this is a remarkable confirmation and illustration of those experiments, in which, by taking the electric spark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times since I first published the account of it, and always with the same result.

This experiment also throws some light upon those in which, by exposing iron to nitrous air,
I pro-

I produced a strong smell of volatile alkali; an experiment which I have also frequently repeated with the same result. The reviving of lead in alkaline air may also help us to conceive how all *acids* should have an affinity both to *phlogiston* and to *alkalies*, which have hitherto appeared to be things so very different from each other; since, from these experiments, it is probable that one of them is some modification of the other, or a combination of something else with the other. To trace the connexion between the alkaline and inflammable principles, is a curious subject; and from these hints it may, perhaps, not be very difficult to prosecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound, and inflammable air, or phlogiston, the more simple substance of the two.

From five ounce measures and a half of alkaline air I got, by means of litharge, seventeen grains of lead, besides some that was dissolved in the mercury, by which the air was confined. There remained two ounce measures and a half, which appeared to be phlogisticated air, and to have no fixed air in it. At another time, in eight ounce measures of alkaline air I got fifteen grains of lead, besides what was dissolved in the mercury, which seemed to be a good deal in proportion to

it. There remained in this process three ounce measures and a half of phlogisticated air, without any mixture of fixed air in it.

Having thus produced *lead* in inflammable air, I proceeded in my attempts to revive other metals from their calces by the same means; and I succeeded very well with tin, bismuth, and silver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arsenic, zinc, or the metal of manganese.

I was desirous also of ascertaining by this means the *quantity* of phlogiston that enters into the composition of the several metals; but in this I found more difficulty than I had expected; and this arose chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not easy to revive the whole of any quantity of calx completely.

After many trials, I think I may venture to say, that an ounce of *lead* absorbs a hundred ounce measures of inflammable air, or perhaps something more; for in one result it seemed to have imbibed in the proportion of 108 ounce measures.

An ounce of *tin* absorbs inflammable air in the proportion of 377 ounce measures to the ounce.

An

An ounce of copper from verditer absorbed 403 ounce measures; from a solution of blue vitriol, precipitated by salt of tartar, and afterwards made red hot with spirit of nitre, 640; but from blue vitriol itself 909 ounce measures. In this case, however, much of the inflammable air went to the formation of the vitriolic acid air, the smell of which was very perceivable in the course of the experiment. The copper that I made in this way was brittle, and therefore seemed not to be perfectly metallized; but being fused with borax, it became perfect copper, and, as I think, without any loss of weight.

Bismuth absorbed inflammable air in the proportion of 185 ounce measures to the ounce. The calx I used was a precipitate from the solution of this metal in spirit of nitre.

Iron I got from a precipitate of a solution of green vitriol by salt of tartar, moistened with spirit of nitre, and exposed to a red heat. This calx absorbed in the proportion of 890 ounce measures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiston, should be produced in this manner, since

inflammable air may be expelled from perfect iron in this very process*.

Silver I evidently revived from a solution of it in spirit of nitre precipitated by salt of tartar, and also from *luna cornea*. A quantity of this last substance absorbed twenty three ounce measures of inflammable air; but I could not get any calx of silver free from small grains of the perfect metal, which was easily discovered by a magnifier, and therefore I could not ascertain the quantity of inflammable air absorbed by it.

Small grains of regulus of *cobalt* I produced from zaffre, and inflammable air was absorbed; but I did not estimate the quantity.

A quantity of *manganese* absorbed seven ounce measures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpose; and mixing with it some calcined borax, I repeated the experiment, when there was again an evident absorption of air, and in the course of that experiment, I once thought that I did perceive a small globule of metal.

Zinc and *arsenic* were only sublimed in this process. The same was the case with the glass of

* I have since found that inflammable air cannot be expelled from iron by heat, without some moisture, which therefore seems necessary to its constitution.

antimony;

antimony; but the experiment was attended with this peculiar circumstance, that when the glass was melted in inflammable air, it formed itself into needle-like crystals, arranged in a very curious manner; and I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a sufficient proof of its *containing* what has been called phlogiston; and its being absorbed by them *in toto*, without decomposition, is a proof that, exclusive of water, it is nothing besides *phlogiston in the form of air*, unless there should be something solid deposited from it at the same time that the proper phlogistic part of it is absorbed. With respect to this, I can only say that, in the course of the experiments, I did not perceive any thing of the kind: for though in some of the processes there was a black smoke produced, in others I could perceive nothing but part of the calx subliming, and clouding the glass. On this account, however, I could not pretend to ascertain the weight of the inflammable air in the calx, so as to prove that it had acquired an addition of weight by being metalized, which I often attempted. But were it possible to procure a perfect calx, no part of which should be sublimed and dispersed, by the heat necessary to be made use of in the process, I

should not doubt but that the quantity of inflammable air imbibed by it would sufficiently add to its weight.

Besides the formation of metals from their calces, I had other proofs, and of a nature sufficiently curious, of inflammable air containing phlogiston. Thus, by means of it, I was able to make *phosphorus*, *nitrous air*, *liver of sulphur*, and *sulphur* itself, in all of which phlogiston is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glassy matter which is made from calcined bones by oil of vitriol in inflammable air, some of it was absorbed, and all the inside of the receiver was covered with an orange coloured substance, which had a strong smell of phosphorus. I then wanted sun-shine to continue the experiment; but I was satisfied that there was sufficient proof of phosphorus being actually formed in this manner. With alkaline air I succeeded much better.

In two ounce measures and a half of this air, I produced, from the glassy matter mentioned above, two grains of phosphorus in one mass, the vessel being only filled with white fumes during the process. One fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the process for making phosphorus.

That nitrous air contains phlogiston is sufficiently evident, if there be any such thing as phlogiston: and I have farther proved, that it contains very nearly as much phlogiston, in proportion to its bulk, as inflammable air itself. I had now, however, the farther satisfaction to be able to make nitrous air from its two constituent principles, viz. nitrous vapour and inflammable air. The most easy process for this purpose is, to throw a stream of nitrous vapour into a large phial previously filled with inflammable air. In this manner nitrous air is instantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid solution of bismuth in spirit of nitre, which at the same time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the same thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I first produced by uniting nitrous vapour to minium (in consequence of which, from being a red and powdery substance, it becomes white, compact, and brittle) I placed it upon a stand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two thirds of the whole, and during this time lead was revived from the

calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all strongly nitrous: and, from the circumstances in which it was produced, it must have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver. In order to ascertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the space of 1.32 measures. Fresh nitrous air made in the usual way, and mixed with common air in the same proportion, occupied the space of 1.26. This difference arose not from any impurity in the nitrous air, but from the mixture of the dephlogisticated air, which is also expelled from this calx by heat.

Liver of sulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Lastly, to produce *sulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen vessel, and evaporated it to dryness in a receiver filled with inflammable air; in consequence of which the inside of the receiver acquired a whitish incrustation, which when warmed had a strong smell of sulphur; and
repeating

repeating the process in the same receiver, I was able, this second time, to scrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar smell, of sulphur.

P A R T III.

OF THE CONSTITUTION OF INFLAMMABLE AIR,

SECTION I.

Experiments which prove that Water is a necessary ingredient in inflammable Air.

A T first I had no suspicion that water was any part of inflammable air, and it may be worth while to recite the experiments which led to that conclusion. Having put a quantity of *iron-filings*, carefully sorted with a magnet, into one of the glass-vessels, fig. *a*, Pl. iv. I filled the rest of the vessel with quicksilver; and placing it inverted in a basin of quicksilver, I threw the focus of the lens upon the iron-filings, and presently air was produced; which, being examined, appeared to be inflammable, though
not

not very strongly so. It resembled inflammable air that had been washed in water till its inflammability was nearly gone. I also could not distinguish the colour of the flame, when I made the explosion in the usual manner, by the approach of a candle. After the operation, the iron from which the air had been extracted, had an exceedingly strong smell, exactly like that of very strong inflammable air procured from metals by acids.

In the same manner I got air from the *filings* of *watch-springs* which are made of the best of steel; and it was not to be distinguished from the inflammable air of the last experiment. These filings, as well as those of iron, I had carefully sorted with a magnet, so that I believe there was no foreign matter mixed with them.

N. B. The spot on which the focus of the lens was thrown, was much blacker than any other part of the filings; and during the application of the heat, a quantity of the filings would sometimes be dispersed, as by an explosion below the surface of them; owing, I suppose, to the sudden generation of air from some of the filings that lay under the rest, but where the heat could reach them.

Having thus got air from *iron*, I proceeded to make similar experiments on other metals. But as all the other metals have more or less affinity with quicksilver, I was obliged to have recourse to a

vacuum;

vacuum. But being possessed of Mr. Smeaton's air-pump, I could depend upon the vacuum being very exact; so that very little common air could be mixed with the air produced. That the filings of the different metals might be perfectly unmixed, I procured new files, quite clean, and used one side of each for each of the metals.

With this apparatus, I threw the focus of my lens upon filings of *zinc*, and presently got from them air which was very strongly inflammable. Zinc is said to contain more phlogiston than the other metals, and the difference between the inflammable air from zinc, and that which I got from iron, was very striking.

From *brass-dust* I got inflammable air in considerable plenty, and also from tin; but this last was very slightly inflammable. I could not have perceived it to be so at all but by dipping a lighted candle into a vessel full of it; whereas, in other cases, I made the trials by presenting the flame of a candle to the narrow mouth of a phial filled with the air. That brass should yield inflammable air, I attribute to the zinc, by the addition of which, copper is converted into brass.

Thus all the metals that yield inflammable air, when dissolved in acids, gave inflammable air also by heat only. With other metals I had no success.

Regulus of *Antimony*, heated *in vacuo*, smoked very much,

much, and blackened all the inside of my receiver; but the air that I got from it was very little indeed, and extinguished a candle.

From *bismuth*, and *nickel*, I got hardly any air at all; but in these experiments the heat was not advantageously applied, and the bismuth soon melted into large lumps, on which my lens had no power.

I got no air from *lead* or *copper*. By throwing the focus of the lens upon the former, the receiver was filled with fumes; but the heat was by no means sufficient for the experiment with copper.

It is generally said, that charcoal is indestructible, except by a red heat in contact with air. But I found that it is perfectly destructible, or decomposed, *in vacuo*, or as will appear hereafter, by means of *water* which it attracts when red hot from the moisture in the receiver. For in these circumstances, and by the heat of a burning lens it is almost wholly converted into inflammable air; so that nothing remains besides an exceedingly small quantity of white ashes, which are seldom visible, except when, in very small particles, they happen to cross the sun-beam, as they fly about within the receiver. It would be impossible to collect or weigh them; but, according to appearance, the ashes thus produced from many pounds of wood, could not be supposed to weigh a grain. The great weight of ashes produced by burning wood in the open air, arises from
what

what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without the least particle of fixed air in it. But, in order to this, the charcoal must be perfectly well made, or with such a heat as would expel all the fixed air which the wood contains; and it must be continued till it yield inflammable air only, which, in an earthen retort, is soon produced.

Wood, or charcoal, is even perfectly destructible, that is, resolvable into inflammable air, in a good earthen retort, and a fire that would about melt iron. In these circumstances, after all the fixed air had come over, I have several times continued the process during a whole day, in all which time inflammable air has been produced equably, and without any appearance of a termination. Nor did I wonder at this, after seeing it wholly vanish into inflammable air *in vacuo*. A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce measures of inflammable air in twelve minutes.

That water in great quantities is sometimes produced from burning inflammable and dephlogisticated air seemed to be evident from the experiments of Mr. Cavendish and Mr. Lavoisier. I have also frequently collected considerable quantities of water in this way, though never quite so much as the weight of the two kinds of air decomposed. My apparatus,

apparatus for this purpose was the following. Into the mouth of a large glass balloon (*a*) Fig. 4. Pl. vii. I introduced a tube from the orifice of which there continually issued inflammable air, from a vessel containing iron and oil of vitriol. This being lighted, continued to burn like a candle. Presently after the lighting of it, the inside of the balloon always became cloudy, and the moisture soon gathered in drops, and settled in the lower part of the balloon. To catch what might issue in the form of vapour, in the current of air through the balloon, I placed the glass tube (*b*) in which I always found some water condensed. It is very possible, however, that in both these modes of experimenting, the water may be converted into a kind of vapour, which is very different from *steam*, and capable of being conveyed a great way through air, or even water, without condensation, along with the air with which it is mixed; and on this account it may not be possible, in either of these modes of experimenting, to collect *all* the water which the two kinds of air will yield. The nature of this kind of vapour into which water may be changed, and which is not readily condensed by cold, is very little understood, but well deserves the particular attention of philosophers*. Even mercury will evapo-

* Mr. Saussure has made some valuable observations on this subject.

rate,

rate, so as to lose weight, in a degree of heat below that of boiling water.

That the water collected in the balloon came from the decomposition of the air, and not from the fresh air circulated through it, was evident from placing balls of hot iron in the place of the flame, and finding that, though the balloon was as much heated by them as by the flame of the burning of the inflammable air, and consequently there must have been the same current of the external air through it, no moisture was found in the balloon.

When, in this manner, I burned inflammable air from pure iron, the water I collected was as far as I could perceive free from acid, and the inside of the balloon was quite clear, but when I used *sulphorated iron*, there was a dense white cloud that filled the inside of the balloon. There was also a strong smell of vitriolic acid air, and the water collected was sensibly acid to the taste.

Afterward, seeing much water produced in some experiments in which inflammable air was decomposed, I was particularly led to reflect on the relation which they bore to each other, and especially Mr. Cavendish's ideas on the subject. He had told me that notwithstanding my former experiments, from which I had concluded that inflammable air was pure phlogiston, he was persuaded that *water* was essential to the production of it, and even entered into it

it as a constituent principle. At that time I did not perceive the force of the arguments which he stated to me, especially as, in the experiments with charcoal, I totally dispersed any quantity of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no suspicion that the wet leather on which my receiver stood could have any influence in the case, while the piece of charcoal was subject to the intense heat of the lens, and placed several inches above the leather. I had also procured inflammable air from charcoal in a glazed earthen retort two whole days successively, in which it had given inflammable air without intermission. Also iron filings in a gun-barrel, and a gun-barrel itself, had always given inflammable air whenever I tried the experiment.

These circumstances, however, deceived me, and perhaps would have deceived any other person; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intensely hot. They will find, and attract it, in the midst of the hottest fire, and through any pores that may be left open in a retort; and iron filings are seldom so dry as not to have moisture enough adhering to them, capable of enabling them to give a considerable quantity of inflammable air. But my attention being now fully awake to the subject, I presently found that the cir-

circumstances above-mentioned had actually misled me; I mean with respect to the *conclusion* which I drew from the experiments, and not with respect to the experiments themselves; every one of which, I doubt not, will be found to answer, whenever they are tried by persons of sufficient skill and properly attentive to all the circumstances.

Being thus apprized of the influence of unperceived moisture in the production of inflammable air, and willing to ascertain it to my perfect satisfaction, I began with filling a gun-barrel with iron filings in their common state, without taking any particular precaution to dry them, and I found that they gave air as they had been used to do, and continued to do so many hours. I even got ten ounce measures of inflammable air from two ounces of iron filings in a coated glass retort. At length, however, the production of inflammable air from the gun-barrel ceased; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston.

I then repeated the experiment with the charcoal, making the receiver the stand on which I placed the charcoal, and the charcoal itself, as dry and as hot as possible, and using cement instead of a wet leather to exclude the air. In these circumstances I was not
able,

able, with the advantage of a good sun, and an excellent burning lens, to decompose quite so much as two grains of the piece of charcoal, which gave me ten ounce measures of inflammable air; and this I imagine, was effected by means of so much moisture as was deposited from the air in its state of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as necessary as to that from iron.

As inflammable air was produced in some experiments, in which I endeavoured to change the nature of water, by making it red hot in a gun-barrel, the orifice of which was welded up, it may not be improper just to mention them in this place, as they shew the use of water in procuring this kind of air. They will likewise serve to shew the expansive force of water in that state. The experiments were made in March 1783.

Putting sixteen grains of water into a gun-barrel, containing four ounce measures and a half, I got it welded up; and making it red hot, it burst in the middle after a few minutes. I ascertained the quantity of water, by putting it into a small glass tube, which I sealed hermetically, and put within the gun-barrel.

I then put six grains of water into the thicker half of a musket barrel, and three grains and a half

into a thinner barrel. These did not burst when they were red hot, and being pierced under water, inflammable air rushed out. I repeated these experiments, and always had the same result; inflammable air being procured, when the gun-barrels were opened under water; and if the water was in sufficient quantity, part of it at least (for I could not measure it with exactness) was found in the barrel.

If inflammable air always contains water, water should be found whenever this kind of air is decomposed; yet in heating *red precipitate* in inflammable air, I at one time found little or no water. But having used more precautions, I have since found it in sufficient quantity in this process, even though the inflammable air was previously well dried with fixed ammoniac. In this experiment I discontinued the process after three ounce measures of air were absorbed, leaving room in the vessel, that the moisture might be more easily collected. With this precaution, and warming the vessel, I collected between an half and three-fourths of a grain of water.

This experiment may be thought to be favourable to the hypothesis of water being composed of fixed and inflammable air; as all water was carefully excluded, and yet a sufficient quantity was found in the process. But besides taking into the account the water that is necessary to constitute the inflammable air, why may not *red precipitate*, in its driest

driest state, be supposed to contain water, as well as the scales of iron, which will bear any degree of heat without parting with it. Red precipitate is made by a liquid process, and therefore the water, that may enter into its composition as a calx, may quit it when it becomes a metal.

Having found that water is an essential ingredient in the constitution of inflammable air, at least as produced from iron, it still remained to be determined whether, when a calx is revived, and the metal formed, the pure phlogiston only entered the calx, or, together with it, that *water* which was necessary to its form of inflammable air.

In order to ascertain this, I frequently revived dry calces of lead in dry inflammable air, and examined the appearances of moisture afterwards. But notwithstanding all the attention that I gave to the process, I could not be absolutely certain, whether more moisture was left in the vessel than might have existed *extraneously* in the inflammable air, or whether, when the phlogiston was absorbed, it left behind it any water that had been essential to it, as inflammable air. Appearances were such as sometimes inclined me to think that every thing which constitutes inflammable air goes into a calx, in order to form the metal; so that if this, though a compound thing, be called *phlogiston*, it will still be true that phlogiston and inflammable air are the same thing; but, on the whole, I rather think that the

water which was essential to the constitution of inflammable air was left behind.

That water, however, may exist in bodies in a *combined state*, without appearing to be water, we know in many cases; but it is in nothing more evident than in the *scales of iron*, than which no substance can have less the appearance of containing water.

But not to give a mere *opinion*, I shall recite the particulars of a few *experiments*, which I made with the view above-mentioned. In six ounce measures and a half of inflammable air from iron, I revived lead till it was reduced to one ounce measure and a half, care having been taken to make every thing as dry as possible. Some moisture, however, did appear, perhaps more than half a grain; but as this air had been confined by water, it was no more than might have been contained in it as an extraneous substance. It ought also to be considered, that it must be exceedingly difficult to expel all moisture by mere heat from such a powdery substance as the yellow caix of lead, without reviving the metal. All chemists well know how firmly moisture adheres to many substances, with which it does not properly *unite*, and how much heat is necessary to separate them.

Again, in six ounce measures and a half of inflammable air from iron, I revived lead till there remained 0.9 of a measure, and there was hardly any more moisture than I had reason to think might have

have been in the vessel, independently of what was contained in the inflammable air; and in order to enable myself to judge of this, I melted an equal quantity of the same minium, under a dry glass vessel with common air, when a little moisture appeared on the inside of the glass, about as much, I thought (for I could only judge by my eye) as when I had revived the lead from that minium in inflammable air. The quantity of lead revived was only sixteen grains, but a good deal of the minium had been made black in the process.

Lastly, I exposed some calx of lead to the heat of the lens in inflammable air, received immediately from the vessel in which it was generated from iron and oil of vitriol, because this contains less water than that which has been received in water and confined by it; and when six or seven ounce measures of the air were absorbed, I could not suppose, from the appearance, that the water could be more than a quarter of a grain. However, when I repeated the experiment once more, I thought there might be about half a grain of water, which is more than I can well account for, without supposing that the water which was necessary to the constitution of inflammable air, and which I suppose to be about half its weight, was left behind when the pure phlogiston revived the calx. This, therefore, is the

opinion to which I am inclined; so that I do not think that any water enters into the constitution of any of the metals.

SECTION II.

Inflammable Air from Charcoal and Iron, &c. by Means of Steam.*

EVER since the discovery of the diminution of respirable air in those processes which are generally called *phlogistic*, it has been a great object with philosophers to find what becomes of the air which disappears in them.

Mr. Cavendish was of opinion, that when *air* is decomposed, *water* only is produced; and Mr. Watt concluded from some experiments, of which I gave an account to the Royal Society, and also

* This section (which was an article in the Philosophical Transactions, Vol. 75, p. 279) might have been introduced into Part I. which treats of the production of inflammable air; but as it likewise proves the composition of it from *water* and *phlogiston*, it will, upon the whole, stand better in this connexion.

from

from some observations of his own, that water consists of dephlogisticated and inflammable air, in which Mr. Cavendish and M. Lavoisier concur with him; but Mr. Lavoisier is well known to maintain, that there is no such thing as what has been called *phlogiston*; affirming inflammable air to be nothing else but one of the elements or constituent parts of water.

Such were the hypotheses to which I had a view, when I began the following course of experiments, which I hope will be an admonition to myself, as well as to others, to adhere as rigorously as possible to *actual observations*, and to be extremely careful not to overlook any circumstance that may possibly contribute to any particular result. I shall have occasion to notice my own mistakes with respect to *conclusions*, though all the *facts* were strictly as I have represented them. But whilst philosophers are faithful narrators of what they observe, no person can justly complain of being misled by them; for to *reason* from the facts with which they are supplied, is no more the province of the person who discovers them, than of him to whom they are discovered.

I had transmitted the vapour of several fluid substances through red hot *earthen tubes*, and thereby procured different kinds of air. M. Lavoisier adopted the same process, but used an *iron tube*;
and

and by means of that circumstance made a very valuable discovery which had escaped me. I had indeed, on one occasion made use of an iron tube, and transmitted steam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myself with observing that *water*, after being made red hot, was still water, there being no change in its sensible properties. Being now farther instructed by the experiment of M. Lavoisier, I was determined to repeat the process with all the attention I could give to it; but I should not have done this with so much advantage, if I had not had the assistance of Mr. Watt, who always thought that M. Lavoisier's experiments by no means favoured the conclusion that he drew from them. As to myself, I was a long time of opinion that his conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process. But though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. Watt's observations threw upon them, satisfied me at length that the inflammable air came from the charcoal, or the iron.

I shall first relate the result of the experiments that was made with *charcoal*, and then those with iron and other substances, in contact with which
(when

(when they were in a state of fusion, or at least red hot) I made steam, or the vapour of other liquid substances, to pass. I shall only observe that, previous to this, I began to make the experiments with coated glass tubes, which I found to answer very well during the process, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. Lavoisier discovered, steam had no effect; and at last I made use of earthen tubes, with which Mr. Wedgwood, that most generous promoter of science, liberally supplied me for the purpose; and these, glazed on the outside only, I find far preferable to copper.

The disposition of the apparatus, with which these experiments were made, was as follows. The water was made to boil in a glass retort, which communicated with the copper or earthen tube that contained the charcoal or iron, &c. and which, being placed in an horizontal position, was surrounded with hot coals. The end of this tube opposite to the retort communicated with the pipe of a common *worm tub*, such as is generally used in distillations, by means of which all the superfluous steam was condensed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was transmitted into a trough of water,

water, where proper vessels were placed to receive it, and ascertain the quantity of it; after which I could examine the quality of it at leisure*.

In the experiment with *charcoal*, I found unexpected difficulties, and considerable variations in the result; the proportion between the *charcoal* and *water* expended, and also between each of them and the *air* produced, not being so nearly the same as I imagined they would have been. Also the quantity of fixed air that was mixed with the inflammable air varied very much. This last circumstance, however, some of my experiments may serve to explain. Whenever I had no more water than was sufficient for the production of the air, there was never any sensible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the case when I produced the air by means of a burning lens in an exhausted receiver, and also in an earthen retort with the application of an intense heat. I therefore presume, that when the steam transmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwise have been. The extremes that I have observed in the proportion of the fixed to the in-

* The disposition of this apparatus may be seen Pl. VII. fig. 2,

flammable

flammable air have been from one twelfth to one fifth of the whole. As I generally produced this air, the latter was the usual proportion; and this was exclusive of the fixed air that was intimately combined with the inflammable air, and which could not be separated from it except by decomposition with dephlogisticated air; and this combined fixed air I sometimes found to be one third of the whole mass, though at other times not quite so much.

To ascertain this, I mixed one measure of this inflammable air from charcoal (after the uncombined fixed air had been separated from it by lime water) with one measure of dephlogisticated air, and then fired them by the electric spark. After this I always found that the air which remained made lime water very turbid, and the proportion in which it was now diminished, by washing in lime water, shewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this process, is evident from there being no fixed air found after the explosion of dephlogisticated air and inflammable air from iron*.

* When I wrote this paper, I imagined that the *fixed* air, which was found on the decomposition of this inflammable air with dephlogisticated air, had been contained in the inflammable air. But it will appear, that it must have been formed by the union

Notwithstanding the above-mentioned variations, the loss of weight in the charcoal was always much exceeded by the weight of the water expended; which was generally more than double that of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever deposited from it.

The experiment which, upon the whole, gave me the most satisfaction, and the particulars of which I shall therefore recite, was the following. Expending ninety four grains of perfect charcoal (by which I mean charcoal made with a very strong heat, so as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce measures of air, one fifth of which was fixed air, and of the inflammable part nearly one third more appeared to be fixed air by decomposition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have ascertained the quantity of it) consisting of fixed and inflammable air together, I found some variations in its specific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of fourteen

union of phlogiston (or inflammable air) and dephlogisticated air, made by the explosion; though it is remarkable that no fixed air is formed when the inflammable air from iron is used.

grains

grains to forty ounce measures is pretty near the truth, when the proportion of fixed air is about one fifth of the whole. With respect to the weight of the inflammable air after the fixed air was separated from it, I found no great difference, and think it may be estimated at eight grains to thirty ounce measures.

Upon these principles, the whole weight of the 840 ounce measures of air will be 294 grains

that of the charcoal will be	94
that of the water - -	240

334

which, considering the nature of the experiment, will perhaps be thought to be tolerably near to that of the air.

If the air be analyzed, the 840 ounce measures will be found to contain

	168 of uncombined fixed air=151 grains.
	and 672 impure inflammable =179
so that the whole 840 will weigh - -	330

It may, however, be safely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itself may be very well supposed to have supplied.

There is, therefore, no reason for deserting the old established hypothesis of *phlogiston* on account

of these experiments, since the fact is by no means inconsistent with it. The pure inflammable air, with the water necessarily contained in it, would weigh no more than about thirty grains, while the loss of weight in the charcoal was ninety four grains. But to this must be added the phlogiston contained in 392 ounce measures of fixed air, which, according to Mr. Kirwan's proportion, will be nearly sixty five grains, and this and the thirty grains will be ninety five grains.

The basis to this fixed air, as well as to the inflammable, must have been furnished by the *water*; and I afterwards found that water is about one half of the weight of fixed air.

Before I conclude my account of the experiments with charcoal, I would observe, that there is another on which I place some dependence, in which, with the loss of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce measures of air, of which the last portion (for I did not examine the rest) contained one sixth part of uncombined fixed air. This was made in an earthen tube glazed on the outside.

The experiments with *iron* were more satisfactory than those with charcoal, being subject to less variation; and they by no means require us to suppose that the inflammable air comes from the *water*, but only from the *iron*, as the quantity of
water

water expended, deducting the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this process is between one third and one half more than can be procured from iron by a solution in acids, the reason may be, that much phlogiston is retained in the solutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would farther observe, that the produce of air, and also the addition of weight gained by the iron, are much more easily ascertained in these experiments than the quantity of water expended in them, on account of the great length of the vessels used in the process, and the different quantities that may perhaps be retained in the worm of the tub; though I did not fail to use all the precautions that I could think of, to guard against any variation on these accounts.

Of the many experiments which I made with *iron*, I shall content myself with reciting the following results. With the addition of 267 grains to a quantity of iron, and the loss of 336 grains of water, I procured 840 ounce measures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the consumption

of 254 grains of water, I got 420 ounce measures of air*.

The inflammable air produced in this manner is of the lightest kind, and free from that very *offensive smell* which is generally occasioned by the rapid solution of metals in oil of vitriol, and it is extricated in as little time in this way as it is possible to do it by any mode of solution. On this account it occurred to me, that it must be by much the cheapest method that has yet been used of filling *balloons* with the lightest inflammable air. For this purpose it will be proper to make use of cast iron cylinders of a considerable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itself will contribute to the production of air, and therefore may

* If the perfect accuracy of the former of these experiments may be depended on (and it may always be presumed, that those in which *little water* is expended are preferable to those in which *more* is consumed) the water that necessarily enters into this kind of inflammable air is about equal in weight to the *phlogiston* that is in it.

The water expended was 336 grains, and the iron gained 267 grains. Supposing it to have lost phlogiston equal to half the weight of the inflammable air, viz. 840 ounce measures = 25 grains (the whole weight of that air being 50 grains) the water that really entered into the iron must be estimated at 292 grains (which is $267 + 25$). This deducted from 336, leaves a remainder of 34, which is not much more than 25, or half the weight of the inflammable air.

become

become unfit for the purpose in time; yet, for any thing that I know to the contrary, the same tube may serve for a very great number of processes, and perhaps the change made in the inside surface may protect it from any farther action of the water, if the tube be of sufficient thickness; but this can only be determined by experiment.

Having recommended this process as the cheapest and the most convenient for filling balloons, especially when tubes of cast iron should be made use of; I was willing to make a trial of one, in order to form some judgment how long they would last for the purpose. I therefore procured one of an inch and a quarter in diameter, and not more than a quarter of an inch in the thickness of the metal; and making the middle part of it red hot, sent steam through it; and from the result of four or five processes with the same tube, I have little doubt, but that, if they were made of the thickness of half an inch in the metal, and care was taken to coat them on the outside with clay and sand, the same tube might probably serve twenty times. That the reader may form some judgment as well as myself, I shall mention the result of my observations.

I heated the tube four or five different times, and in each process transmitted as much water through it as would have been more than sufficient

to decompose all the iron that it could have contained. At first four ounce measures of water procured 180 ounce measures of inflammable air, and then six ounce measures procured only 160 ounce measures. I then examined the tube, and found that when both the inside and outside were well scraped with a sharp instrument, it had lost twenty five grains in weight. The scales from the outside weighed 282 grains, while all that I could get from the inside weighed only thirty six grains. Consequently the tube had gained in weight 283 grains. After this I heated it again, and transmitted through it six more ounces of water, which yielded only sixty ounce measures of air.

From these experiments it may be inferred, that the tube would soon have ceased to give any air; the inside being changed to some depth by the action of the steam, and yet it was not much disposed to exfoliate. In time it would, no doubt, have become brittle, and might be in danger of breaking, from its disposition to *bend* in the course of the process. This bending was very considerable; but did not seem to arise from any tendency in the iron to *melt*. Perhaps by turning it in cooling, this bending, and consequently the danger of cracking after much use, might be prevented; or this property of bending might in a great measure cease,

cease, when the metallic state of the tube was destroyed; and yet with care might continue a firm and compact tube, and as fit for this process as at the first. If this should be the case (which experience alone can determine) it is not to say how long a tube of this kind might last. It would then be a kind of *earthen tube*, of the most perfect nature, completely air tight, without being subject to rust or decay.

Upon the whole, should the fondness for balloons be resumed, I see no reason why far the greatest part of the expence attending the filling of them might not be saved by means of this process. A complete apparatus for it would not cost half so much as the filling of a single balloon, that would carry a man, in the common way, and would serve at least a considerable number of times, with the expence of a very few pounds each time; as there would be hardly any thing to pay for besides *fire* and *attendance*, for a few hours. For such *iron* as would best answer for this purpose might, in most places, be had for a mere trifle. One apparatus, conveniently fixed, might serve for a whole town or neighbourhood.

Some estimate of what may be expected from this method of procuring inflammable air may be formed from the following observations. About twelve inches in length of a copper tube, three

fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpose than *iron filings*, as they do not lie so close, but admit the steam to pass through their interstices) when it was heated, and a sufficient quantity of steam passed through it, yielded thirty ounce measures of air in fifty seconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the same manner, gave two hundred ounce measures in one minute and twenty five seconds; so that this larger tube gave air in proportion to its solid contents compared with the smaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates so readily to some distance, the rate of giving air will always be in a greater proportion than that of the simple diameter of the tube.

The following experiment was made with a view to ascertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when dissolved in acids, will yield about 800 ounce measures of air; but treated in this manner it yielded 1054 ounce measures, and then the iron had gained 329 grains in weight, which is little short of one third of the weight of the iron.

Considering how little this inflammable air weighs, viz. the whole 1054 ounce measures not more than sixty three grains, and the difficulty of ascertaining the loss of water to so small a quantity as this, it is not possible to determine, from a process of this kind, how much water enters into the composition of the inflammable air of metals. It would be more easy to determine this circumstance with respect to the inflammable air of charcoal, especially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce measures of inflammable air, which, in the proportion of thirty ounce measures to eight grains, will weigh 3.3 grains; so that water in the composition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be some difficulty with respect to the fixed air intimately combined with this kind of inflammable air.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the same result when I made use of small nails of *cast iron*, except that these were firmly fastened together after the experiment, the surfaces of them being crystallized, and the crystals mixed with each other, so that it was with great difficulty that they could be got out of the tube after the experiment; and in general the solid parts

of the nails were broken before they were separated from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means so firmly.

Cast iron annealed (by being kept red-hot in charcoal) is remarkably different from the cast iron which has not undergone that operation, especially in its being, to an extraordinary degree, more soluble in acids. With the turnings of annealed cast iron I made the following experiment. From 960 grains of this iron, and with the loss of 480 grains of water, I got 870 ounce measures of inflammable air, and transmitting steam through them a second time, I got 150 ounce measures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were easily broken and got out of the tube, whereas it had required a long time, and a sharp steel instrument, to clear the tube of the cast-iron nails.

Having made another experiment with *iron*, with as much attention as I could give to it, I shall in the first place mention *that*. From two ounces of iron turnings (which is cast iron annealed) I got in the first instance 985 ounce measures of air, with the loss of 528 grains of water; and the iron, I found, had gained 292 grains in weight. Then making four ounces of water of this residuum pass over the same iron, it gained four grains more, and
all

all the air that I procured was 998 ounce measures. So that (as extreme accuracy is not to be obtained in processes of this kind) it may be said that two ounces of this kind of iron will, in this way, yield 1000 ounce measures of air; whereas by solution in vitriolic acid, it would have yielded about 800. Consequently more air by two fifths may be procured by this new mode of treatment, which alone should recommend it to those who fill balloons.

Having procured water from the scales of iron (by heating them in inflammable air) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I should be able to produce the same effect by heating it with charcoal in a retort; and I had likewise no doubt but that I should be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of these conjectures I was right; but with respect to the latter, I was totally mistaken.

Having made the scales of iron, and also the powder of charcoal very hot, previous to the experiment, so that I was satisfied that no air could be extracted from either of them separately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in
which

which I could give it a very strong heat ; and connected with it proper vessels to condense and collect the water which I expected to receive in the course of the process. But, to my great surprise, not one particle of *moisture* came over, but a prodigious quantity of *air*, and the rapidity of its production astonished me ; so that I had no doubt but that the weight of the air would have been equal to the loss of weight both in the scales and in the charcoal ; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air ; and the inflammable air, which remained when the fixed air was separated from it, was of a very remarkable kind, being quite as heavy as common air. The reason of this was sufficiently apparent when it was decomposed by means of dephlogisticated air ; for the greatest part of it was fixed air.

The theory of this process I imagine to be, that the phlogiston from the charcoal reviving the iron, the water with which it had been saturated, being now set loose, affected the hot charcoal as it would have done if it had been applied to it in the form of *steam* as in the preceding experiments ; and therefore the air produced in these two different modes have a near resemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions ; and in both the cases I found these proportions subject to variations. In

one

one process with charcoal and scales of iron, the first produce contained one fifth of uncombined fixed air, the middle part one tenth, and the last none at all. But in all these cases the proportion of combined fixed air varied very little.

Why *air* and not *water* should be produced in this case, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to understand. There is, indeed, an obvious difference in the circumstances of the two experiments; as in that with charcoal the phlogiston is found in a combined state; whereas in that of inflammable air, it is loose, or only united to water; and perhaps future experiments may discover the operation of this circumstance*.

* This experiment seems to be decisive against the hypothesis of Mr. Lavoisier, and others, who say that the inflammable air procured by means of iron and charcoal, comes from the water, and who think that by this means they can exclude phlogiston. For, according to them, neither the scales of iron, nor the charcoal, contain phlogiston, or any thing from which inflammable air can be made, but are merely substances capable of imbibing pure air, and thereby setting at liberty the inflammable air contained in the water; supposing the scales of iron to have been only iron saturated with dephlogisticated air. But had this been the case, there was nothing in either of the materials made use of in this experiment from which the inflammable air could possibly come, there being no *water* contained in either of them. But supposing the reality of phlogiston, and its constituting a part of metals, of charcoal, and of inflammable air, the experiment is very intelligible.

After

After having transmitted steam in contact with *charcoal* and *iron* in a copper tube, I proposed to do the same with other substances containing phlogiston, and I began with *bones*, which were burned black, and had been subjected to an intense heat, covered with sand, in an earthen retort. From three ounces of bones thus prepared, and treated as I had done the charcoal, I got 840 ounce measures of air, with the loss of 288 grains of water. The bones were by this means made perfectly white, and had lost 110 grains of their weight. As the air ceased to come a considerable time before all the water had been transmitted through the tube containing them, I concluded that the air was formed from the phlogiston contained in the bones, and so much water as was necessary to give it the form of air.

This air differs considerably from any other kind of inflammable air, being in several respects a medium between that from charcoal and that from iron. It contains about one fourth of its bulk of uncombined fixed air, but not quite one tenth intimately combined with the remainder. The water that came over was blue, and pretty strongly alkaline, which must have been occasioned by the volatile alkali not having been intirely expelled from the bones in the former process, and its having in part dissolved the copper of the tube in which the experiment was made.

I sub.

I subjected to the same process a variety of substances that are said not to contain phlogiston, but I was never able to procure inflammable air by means of them; which strengthens the hypothesis of the principal element in the constitution of this air having been derived from the substance supposed to contain phlogiston, and therefore that phlogiston is a real substance, capable of assuming the form of air by means of water and heat.

SECTION III.

Of the Action of Steam on various Substances in a red Heat.

HAVING procured inflammable air by sending steam over red hot *iron*, I afterwards extended the same process to other substances, and as most of those contained phlogiston, and yielded inflammable air, I shall recite them in this place.

Having been able to decompose iron by converting it into scales, I also found that in this way, I could readily procure *flowers of zinc*, as well as inflammable air from that metal. The flowers came
over

over in a very attenuated state, the air being loaded with them. It might be possible, however, to contrive an apparatus to collect them.

Brafs being made with a mixture of zinc and copper, and *zinc* being so easily decomposed by steam, whenever a copper tube is ordered for the purpose of these experiments, particular care should be taken that there be no mixture of brafs in it, though it is difficult to have copper cast smooth and solid without a mixture of either brafs or tin, which is not much better. Even pure copper tubes become brittle, and at length crack in these experiments.

Having at one time been persuaded to have a little brafs mixed with the copper in one of these tubes, I consented that the smallest quantity that could be supposed to be necessary to make the tube compact, should be put into it. But notwithstanding this, and though the tube had a quarter of an inch thickness of metal, it fell to pieces in the very first experiment, in which I sent the steam of no more than two or three ounces of water through it. Inflammable air was produced very copiously, and the flowers of zinc were mixed with it; so readily did the steam separate the zinc from the copper, though the heat was only sufficient to make the tube red hot, and was far from melting it.

Lead.

Lead would probably be far less affected than copper in these experiments ; but then it will not bear a red heat without melting. I made the steam of about four ounces of water pass over four ounces of melted lead, in an earthen tube, with hardly any sensible effect. The loss of water was not more than 0.2 of an ounce measure.

After using *charcoal* in the experiments recited in the preceding section, I went through one process with *coak*, or the cinder of pit coal, and found that from 174 grains of coak, and with the loss of 528 grains of water, I got 1700 ounce measures of air, of which one fifth was fixed air, and thirty ounce measures of it weighed ten grains less than an equal bulk of common air. The analysis of this air will be found in the section appropriated to that subject.

On *iron ore* this process had no effect. The same was the case with the trial I made of *quick lime*, and such would probably be the case with dephlogisticated earths in general.

With *manganese*, however, the result was different. Having made the steam of four ounces of water pass over 828 grains of this substance, which had been exposed to a strong heat in an earthen retort some time before, I got thirty five ounce measures of air, almost the whole of which was pure fixed air, with a residuum a little better than common

mon air. The manganese had lost 132 grains, and from being *black*, was become very *brown*. Again, I transmitted the steam of eight ounces of water over two ounces and a half of manganese, and got about 100 ounce measures of pure fixed air, with a residuum a little phlogisticated. The manganese had lost 112 grains.

SECTION IV.

*Whether inflammable or nitrous Air contain more
Phlogiston.*

IT is well known that both nitrous and inflammable air contain phlogiston, but in very *different states*, because their specific gravities, and other properties, are most remarkably different. Many schemes have occurred to me to ascertain the proportion of phlogiston that each of them contains, and at length I thought of attempting the solution of this problem, by burning inflammable air in a given quantity of common air. For though inflammable air will not part with its phlogiston to common air when *cold*; it will, like other combustible

tible substances, when heated to a certain degree. It is then decomposed, and the phlogiston that entered into its composition phlogisticates the air in which it is burned; and the degree of phlogistication may be measured by the test of nitrous air. I, therefore, proceeded as follows.

In an eight ounce phial, containing many nails, and a quantity of water with oil of vitriol, I produced inflammable air; and making it burn with a small flame, at the orifice of a glass tube through which the air was transmitted (being cemented into the cork of the phial) I covered the flame with a receiver that contained twenty-one ounce measures of air, standing in water. After six minutes, the flame went out; when, immediately catching the air that was produced in the next six minutes, and also in the six minutes following, I concluded that seven ounce measures had been produced, and decomposed, during the six minutes in which it had continued to burn.

Then examining the air in which it had burned, I found it so far phlogisticated, that equal measures of it and of nitrous air occupied the space of 1.65 measures; and common air mixed with one third as much nitrous air, being again mixed in equal proportions with the same fresh nitrous air, occupied the space of 1.68 measures. It appeared, therefore, that the twenty one ounce measures of air, having

received the phlogiston of one third as much inflammable air, *viz.* seven ounce measures, was about as much phlogisticated as it would have been with a mixture of the same proportion of nitrous air. Consequently, equal measures of nitrous and inflammable air contain about equal quantities of phlogiston.

Of this curious problem, however, I have obtained a more accurate solution from the mode of experimenting introduced by that excellent philosopher Mr. Volta ; who fires inflammable air in common air, by the electric spark, and consequently can determine the exact proportion of the inflammable air decomposed in a given quantity of common air. The result of this process agreeing with that of the former, leaves little doubt with respect to the conclusion I have drawn from them.

Having prepared a strong glass tube, in one end of which I had cemented a piece of wire, I filled it with water, and introduced into it another piece of wire, so as to come within about half an inch of the former wire, that an electric explosion might easily pass between them.

Into this tube, thus prepared, I transferred, in the first place, one measure of inflammable air, and three of common air ; and then, by means of an electric explosion between the wires, in the central place of the air, I fired all the inflammable air,
which

which would then be decomposed, and, of course, part with its phlogiston to the common air with which it was mixed. After the explosion, I accordingly found it to be completely phlogisticated. This also would have been the consequence of mixing the same proportion of nitrous air with the common air. But to determine the problem with accuracy, it was necessary to use such a proportion of inflammable as would only phlogistificate the common air in part.

I therefore mixed one measure of inflammable air with *three* measures of common air, and after the explosion found it to be so far phlogisticated, that one measure of this and one of nitrous air occupied the space of 1.8 measures; and this I also found, by the same test, to be exactly the state to which a mixture of one measure of the same nitrous air brought three measures of the same common air.

In order to obtain a farther confirmation of my conclusion, I mixed one measure of inflammable air with *four* measures of common air; and after the explosion I also found, by the test of nitrous air, that it was phlogisticated exactly as much as by the mixture of an equal quantity of nitrous air. And repeating the experiment with the same proportion of inflammable and common air, I found that after the explosion the air was diminished, without mixing with nitrous air, just as much as one mea-

ture of nitrous air diminished four measures of common air, viz. from 7.4 to 5.2 measures.

Having since this given more attention to these experiments, I have seen reason to conclude that inflammable air from iron and water, contains more phlogiston than nitrous air, in the proportion of ten to nine. For nine measures of inflammable air will diminish dephlogisticated air as much as ten of nitrous air.

SECTION V.

The Analysis of different Kinds of inflammable Air.

BEFORE I proceed to the analysis of the different kinds of inflammable air, which is the subject of this section, I shall observe, that the purest we can procure (which is that from metals by solution in the mineral acids, or rather that by means of steam from red-hot iron) seems to consist of phlogiston and water, and that neither *acid* nor *alkali* is a necessary ingredient in it; though, when
it

it is produced by means of either of them, a small portion of either may be retained in it, as an extraneous substance. That this, however, is the case, has been very clearly shewn by Mr. Senebier, though I think that the production of inflammable air by means of iron and steam only, without either acid or alkali, sufficiently proves that his hypothesis of inflammable air necessarily acquiring some saline basis, cannot be well founded.

It was, indeed, my own first opinion, that inflammable air consists of acid and phlogiston. Afterwards I adopted the opinion of Mr. Kirwan, viz. that it is pure phlogiston in the form of air, but at present I am fully satisfied with the opinion of Mr. Cavendish, that *water* is an essential ingredient in the constitution of this kind of air.

That no acid is necessarily contained, or at least in any sensible quantity, either in inflammable air, though produced by means of acids, or in the dephlogisticated air of the atmosphere, seemed to be evident from the following experiment, which I made with the greatest care. Taking a basin which contained a small quantity of water tinged blue with the juice of turnsole, I placed in it a bent tube of glass, which came from a vessel containing iron and diluted oil of vitriol; and lighting the current of inflammable air, as it issued from this tube, so that it burned exactly like a candle, I placed over

it an inverted glass jar; so that the mouth of it was plunged in the liquor. Under this jar the inflammable air burned as long as it could, and when it was extinguished, for want of more pure air, I suffered the liquor to rise as high as it could within the jar, that it might imbibe whatever should be deposited from the decomposition of either of the two kinds of air. I then took off the jar, changed the air in it, and lighting the stream of inflammable air, replaced the jar as before. This I did till I had decomposed a very great quantity of the two kinds of air, without perceiving the least change in the colour of the liquor, which must, I thought, have been the case if any acid had entered as a necessary constituent part into either of the two kinds of air. I also found no acid whatever in the water which was procured by keeping a stream of inflammable air constantly burning in a large glass balloon, through which the air could circulate, so that the flame did not go out.

With respect to inflammable air itself, I have before observed, that when sufficient care is taken to free it from any acid vapour that may be accidentally contained in it, it is not in the smallest degree affected by a mixture of alkaline air. On the whole, therefore, I have at present no doubt but that pure inflammable air, though it certainly contains *water*, does not necessarily contain any acid. Yet an acid
vapour

vapour may be easily diffused through it, and may perhaps in many cases be obstinately retained by it, as no kind of air seems to be capable of so great a variety of impregnations as inflammable air is.

That there are different kinds of inflammable air, has been observed by most persons who have made any experiments on air. That which has been most commonly observed is, that some of them burn with what may be called a *lambent flame*, sometimes blue, sometimes yellow, and sometimes white, like the flame from wood or coals in a common fire; whereas another kind always burns with an *explosion*, making more or less of a report, when a lighted candle is dipped into a jar filled with it. Of the latter kind is that which is extracted from metals by means of acids, &c. and of the former kind is that which is expelled from wood, coal, and other substances by heat. It has also been observed, that these kinds of inflammable air have different specific gravities, the purest kind, or that which is extracted from iron, &c. being about ten times lighter than common air, but some of the other kinds not more than twice as light.

The cause of this difference I once thought I had discovered to be the heavier kinds of inflammable air containing a proportion of *fixed* air, so intimately combined with them, so as not to be

discoverable by lime water, while the lightest kind contained no fixed air at all. This hypothesis I formed from decomposing them with common or dephlogisticated air, by the electric explosion. For, after the experiment with the heavier kinds of inflammable air, I always found a quantity of fixed air in the residuum, but none at all after the experiment with the lightest kind.

In order to decompose any kind of inflammable air, I generally mix it with an equal quantity of dephlogisticated air, and then confine them in a strong glass vessel, previously filled either with water or mercury, and I make an electric spark in some part of the mixture, by means of wires inserted through the sides of the vessel, and nearly meeting within it. By this process, I imagined, that I was able to ascertain two things relating to the constitution of different kinds of inflammable air, viz. the quantity of *combined fixed air* (as I then thought it to be) and likewise the relative quantity of phlogiston contained in each of them. The former appeared by washing the air with lime water after the explosion, and observing how much of them was absorbed, and the latter by examining the residuum with the test of nitrous air, and observing the purity of it. In most of these experiments I made use of dephlogisticated air, in preference to common air, because I could
not

not make some kinds of inflammable air to explode at all with common air. Otherwise I should have preferred this, as being the most nearly of the same quality. However, I always noted the degree of purity of the dephlogisticated air that I made use of before I began any of these analyses.

Finding, however, that in some cases more fixed air was found after the explosion of the two kinds of air, than could possibly have been *contained* in the inflammable air, on account of the weight of it, I was satisfied that there must have been a real *generation* of it, by an union of the inflammable and dephlogisticated air. It is remarkable, however, that some kinds of inflammable air should so readily unite with dephlogisticated air, so as to make a considerable quantity of fixed air; and that others, treated in the same manner, should not do this at all; and also that those which do it should be the heavier kinds of inflammable air. This is a new and curious subject of investigation.

The purest and lightest inflammable air is that which is extracted from iron, and other metals, by a solution in the acids, or by means of steam. One measure of this kind of air, and one of dephlogisticated (such as that, when mixed with two equal quantities of nitrous air, there remained 0.72 of a measure) exploded together in the manner described above, were reduced to 0.6 of a measure,

ture, no fixed air was found in the residuum, and when examined with an equal quantity of nitrous air, was reduced to 0.87 of a measure.

With the same dephlogisticated air I examined inflammable air that had been got from a mixture of finery cinder and charcoal, and found, that after the explosion, the two measures were reduced only to 1.85, but that by washing in the lime water, they were reduced to 1.2. Consequently 0.65 of a measure of fixed air had been generated in the process. When this was separated from it, and the remainder examined by nitrous air, it appeared to be of the standard of 0.9; so that the dephlogisticated air had been more injured by this than by an equal quantity of the common inflammable air, though the difference in this respect was not considerable.

In another process with this kind of inflammable air, the diminution after the explosion was to 1.55, and that after the washing in lime water to 0.65 of a measure; so that there had been a generation of 0.9 of a measure of fixed air. In another experiment the first diminution was to 1.6, and the second 0.66, so that 0.94 of a measure of fixed air had been produced. And lastly, in another process, the first diminution was to 1.6, and the second to 0.6 of a measure; so that there was a generation of one complete measure of fixed air,
and

and this was a clear proof that it could not have been *contained* in a combined state, as I at first imagined, in the inflammable air; since then it must have been much heavier than I had ever found it to be; for, though I found the specific gravity of it to be something different at different times (and the preceding experiments were made with the air of different processes) I had never found that forty ounce measures of this air was more than two grains heavier than an equal bulk of common air.

This, indeed, is a remarkable circumstance with respect to a species of inflammable air, as it does not appear by the test of lime water to contain any fixed air; but it ought to have weighed more than one half heavier than common air, to have actually contained in combination all the fixed air that I found after its explosion with the dephlogisticated air. Indeed, if any quantity of inflammable air, of about the same specific gravity with common air (which is the case with that species of it which I am now considering) yield so much as seven tenths of its bulk of fixed air, in consequence of its explosion with dephlogisticated air, it is a proof that at least part of that fixed air was generated in the process, because seven tenths of such fixed air would weigh more than the whole measure of the inflammable air.

Inflammable air from *spirit of wine* (made by transmitting it *in vapour* through a red hot earthen tube) being analized in the manner above-mentioned, one measure of it, and one of the same dephlogisticated air that was used in the former experiment, were reduced to one measure, and by washing in lime water to 0.6 of a measure; so that four tenths of its bulk of fixed air had been generated in the process. The standard of the residuum was 1.7; so that the dephlogisticated air had been injured much more than in either of the former processes, and consequently it must have contained more phlogiston.

I found considerable variations in the experiments with this, as well as with some other kinds of inflammable air. For, in another process, in which the earthen tube had been filled with bits of crucibles (in order to expose more red surface to the vapour of the spirit of wine) the first diminution was to 1.6, the second to 1.4; and the standard of the residuum was 1.84. In another process with this kind of air, the first diminution was to 1.2, and the second to 0.9.

Having procured a quantity of inflammable air, by transmitting steam over red hot *platina*, I analized it in the same manner, and found that the two measures were reduced by the explosion to 0.72. It contained no fixed air, and the residuum was of the standard of 0.9.

Inflam.

Inflammable air, procured by making steam pass over melted brimstone, being examined in the same manner, the first diminution was to 0.6, and no fixed air was found in it. In this respect it seems to have been the same thing with inflammable air from iron, but the standard of the residuum was 0.95; so that it seems to have contained more phlogiston. But as the quantity of this air was not great, it probably contained a mixture of other air.

Inflammable air procured in the same manner from melted *arsenic*, appeared to be very different from that which was extracted from brimstone. For the two measures were reduced by the explosion to 1.15, and by washing with lime water, to 0.95; so that one fifth of its bulk of fixed air had been generated. The standard of the residuum was 0.82.

At the same time I examined some inflammable air, that had been made by heating bits of crucibles in *alkaline air*, and found that the two measures were reduced by the explosion to 0.96 of a measure, that the residuum contained no fixed air, and was of the standard of 0.8.

The inflammable air that is made from *ether*, by transmitting the vapour of it through a red hot earthen tube, very much resembles that which is got from spirit of wine. The two measures were reduced by the explosion to 1.36, and by washing in water to 1.2; so that 0.16 of a measure of fixed air had been generated, and the residuum was of the standard of 1.9.

Inflam-

Inflammable air procured by transmitting steam over red hot *charcoal of metals*, in the same manner as it is got from other charcoal, produces a considerable quantity of fixed air. For when the experiment was made with this air, the first diminution was to 1.12, and the second to 0.8; so that 0.32 of a measure of fixed air was generated, and the standard of the residuum was 1.9. This analysis was of the first portion that came in the process. The second was something different. For with this, the first diminution was to 1.0, and the second to 0.75, the residuum being the same as before, viz. 1.9. Thirty ounce measures of this air weighed eight grains less than an equal bulk of common air.

Analizing the inflammable air from *coak*, or the charcoal of pitcoal produced by steam, the first diminution was to 1.15, and the second to 0.95; so that one fifth of its bulk of fixed air was generated. The standard of the residuum was 1.9. But I must observe, that the dephlogisticated air with which this experiment was made, was so impure as hardly to deserve the name. For two measures of nitrous air and one of this, occupied the space of two measures. But this circumstance may not affect the quantity of fixed air generated in the process. Thirty ounce measures of this air weighed ten grains less than an equal bulk of common air.

Analizing the inflammable air that was produced in the same manner from *spirit of turpentine*, the first diminu-

diminution was to 1.7, and the second to 1.6; so that only one tenth of fixed air was produced. The residuum was of the standard of 1.9. Thirty ounce measures of this air weighed eight grains less than an equal bulk of common air.

When the vapour of spirit of wine was made to pass over melted metals, inflammable air was produced from it just as if no metals had been concerned; but when I examined the air that was procured in this manner, it did not appear to be quite the same with that which came from pure spirit of wine. Analyzing the air that was produced in a process with *copper*, the first diminution was to 1.7, the second to 1.56, and the standard of the residuum was 1.78. Thirty ounce measures of this air weighed seven grains less than an equal bulk of common air.

In another process with inflammable air, procured in this manner, the first diminution was to 1.55, the second to 1.48, and the residuum was of the standard of 1.86. Thirty ounce measures of this air weighed eight grains and a half less than an equal bulk of common air. This air, it is observable, produced much less fixed air than the other, and it was also specifically lighter than it.

When this process was made with the air procured by transmitting vapour of spirit of wine over melted *silver*, the first diminution was to 1.9, the
second

second to 1.78, and the standard of the residuum was 1.9. Thirty ounce measures of this air weighed eight grains less than an equal bulk of common air.

In the analysis of the air procured by this process from *lead*, the first diminution was to 1.78, the second to 1.6, and the standard of the residuum was 1.78.

I examined, at the same time, inflammable air procured from *bones*, and also from *charcoal*, viz. by transmitting steam over them when they were red hot in earthen tubes, after all air had been previously expelled from them by heat. With the former, the first diminution was to 0.67, and the second to 0.58; so that the fixed air produced was extremely inconsiderable, viz. only 0.09 of an ounce measure. The standard of the residuum was 1.47. In the experiment with the air from *charcoal*, the first diminution was to 1.5, and the second to 0.74; so that the fixed air was 0.76, and the standard of the residuum was 1.7. From this experiment it may be inferred, as mentioned before, that inflammable air from *bones*, is a kind of medium between that from *metals*, and that from *charcoal*. In another process with air from charcoal, the first diminution was to 0.82, and the second to 0.63, and the standard of the residuum was 1.37.

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I made the following experiment to ascertain how much phlogiston is contained in inflammable air from charcoal. In five ounce measures of this kind of air, I revived lead from massicot till it was reduced to three fourths of an ounce measure, when the lead revived weighed ten grains, and there remained one ounce measure of fixed air. But the minium itself yielded a little fixed air.

It is observable, that when wood is heated in an earthen retort, the first air that comes over is considerably different from that which comes in the middle, or at the end of the process. Indeed the properties of it are continually changing during the whole process. The first portion burns with a lambent white flame, like that from burning wood in an open fire; afterwards the flame is blue, and towards the end of the process it is considerably explosive, almost like air from iron. Also the air that comes over first is very turbid, owing perhaps, to oily, and other matters, that are rendered volatile by heat.

Having procured air from dry beech wood, I examined, in the method described above, the first portion of it, and also one of the middle ones. The former I found to contain four tenths and a half of its bulk of uncombined fixed air, the second portion only two tenths. Afterwards it is well

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known that air procured in this manner ceases to have any uncombined fixed air in it.

When I examined the first portion of air, after the uncombined fixed air had been separated from it, the first diminution was to 1.36, and the second to 0.9; so that 0.46 of a measure of fixed air was generated, and the standard of the residuum was 1.9. When the second portion was examined, after the uncombined fixed air was likewise separated from it, the first diminution was to 1.66, and the second to 1.46; so that the fixed air generated in the process, was 0.2 of a measure, that is, less than in the former experiment, and in nearly the same proportion as the uncombined fixed air had been. The standard of the residuum in this last case was 1.15. At the same time, repeating the experiment with the same dephlogisticated air and inflammable air from iron, the diminution after the explosion was to 0.55, and the standard of the residuum was 1.48, which is the usual result of the decomposition of inflammable and dephlogisticated air, when both of them are as pure as they are generally procured.

There was a great quantity of fixed air produced by the decomposition of some inflammable air extracted from some rich *mould* in a gun barrel, which Mr. Young was so obliging as to send me.

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It burned with a lambent blue flame, and had a peculiarly offensive smell, the same, as he observes to me, that is yielded by air procured from putrid vegetables. Of this air one twentieth part is uncombined fixed air. When this was separated from it, and the remainder decomposed with dephlogisticated air, the first diminution was to 1.4, and the second to 0.67; so that there was a generation of 0.73 of a measure of fixed air. The residuum was of the standard of 0.6.

The inflammable air that is procured from *cast iron* has a peculiarly offensive smell. On this account I had imagined that it might contain more phlogiston than common inflammable air, so as to absorb more dephlogisticated air than the other. But this did not appear to be the fact. For when I mixed one measure of each of the kinds of inflammable air with four measures of common air, the diminution after the explosion, was the very same with both, viz. to 1.56.

Though I think it to be unquestionable, from the preceding experiments, that part at least of the fixed air which is found on the decomposition of lambent inflammable air is *generated* in the process; yet, in another experiment that I made, it should seem that fixed air, or the elements, as we may say, of fixed air, may enter into the composition

of inflammable air, and actually remain there, without being discoverable by lime water.

I took a quantity of *slaked lime*, which had been long kept close corked in a bottle, and found that when it was heated in an earthen retort, it gave air, of which one fifth was for the most part fixed air; but in the gun barrel the same lime yielded no fixed air at all, but a great quantity of pure inflammable air, of the explosive kind, like that which is got from iron alone with water. That the water in slaked lime will enable the iron of the gun barrel to yield inflammable air cannot be questioned, but then what became of the fixed air which the same lime would have yielded in an earthen retort?

This experiment appearing rather extraordinary, I repeated it with all the attention I could give to it, and had the following result. I heated three ounces of slaked lime (but which had been some time exposed to the open air) in an earthen tube, and got from it fourteen ounce measures of air, of which only two measures and a half remained unabsorbed by water, all the rest being fixed air. This residuum was slightly inflammable, but not perfectly phlogisticated. For, examining it with the test of nitrous air, the standard of it was 1.6.

Imme-

Immediately after this I heated another three ounces of the same flaked lime in a gun barrel, and got from it about twenty ounce measures of air, of which no part was fixed air, but all inflammable. I expected, however, to have found fixed air on the decomposition of this inflammable air with dephlogisticated air; but after this process it appeared to be exactly such inflammable air as is procured from metals by the mineral acids, or rather by steam. For the diminution of the two kinds of air was the same, and though there was some appearance of fixed air in the residuum, it was not so much as is found after the decomposition of the inflammable air that is procured by means of spirit of salt. In this case, therefore, there are no less than eleven ounce measures and a half of fixed air absolutely unaccounted for, unless it be supposed that it was resolved into its constituent principles, phlogiston or dephlogisticated air, and that the latter was decomposed as it was produced. This, therefore, I think must have been the case.

Thinking that the two kinds of air might incorporate when one of them was generated within the other, I filled a gun barrel previously full of mercury with fixed air, and put the closed part of it into a hot fire. Inflammable air was accordingly produced, but when the fixed air was separated

from it, it exploded just like inflammable air from iron only.

I made an experiment something similar to this, by heating iron turnings in five ounce measures of fixed air, when the quantity of it was increased about one ounce measure, and there remained one ounce measure and three fourths unabsoꝛbed by water. This was inflammable, and burned with a lambent blue flame, not like inflammable air from iron. It should seem, therefore, that in this experiment, three fourths of an ounce measure of inflammable air had been formed by the union of the fixed air with the phlogiston from the iron. This experiment I repeated with the same result, and I farther observed, that though the inflammable air procured in this manner did not appear, by the test of lime water, to contain any fixed air, yet when it was decomposed, by being fired together with an equal quantity of dephlogisticated air, fixed air was found in the residuum. For the first diminution was to 1.45, and one third of this residuum was fixed air. From this fact it should seem that, though in some cases, fixed air must be *generated* by the decomposition of inflammable and dephlogisticated air, yet that inflammable air, when thus produced in contact with fixed air, may *combine* with it, so as to be properly *contained* in it, and

and in such a manner, as that it cannot be discovered by lime water.

I also observed, after Mr. Metherie, that though no fixed air be found on the decomposition of dephlogisticated air with inflammable air procured by means of oil of vitriol, a small quantity is produced when the inflammable air procured by means of spirit of salt. I did not find, however, more than a fortieth part of the residuum to be fixed air, when I decomposed equal quantities of the two kinds of air.

B O O K III.

EXPERIMENTS AND OBSERVATIONS RE-
LATING TO NITROUS AIR.

P A R T I.

OF THE SOURCE OF NITROUS AIR.

SECTION I.

Of nitrous Air from Metals.

EVER since I first read Dr. Hales's most excellent *Statical Essays*, I was particularly struck with that experiment of his, of which an account is given, Vol. I. p. 224, and Vol. II. p. 280, in which common air, and air generated from the Walton pyrites, by spirit of nitre, made a turbid red mixture, and in which part of the common air was absorbed;

forbed ; but I never expected to have the satisfaction of seeing this remarkable appearance, supposing it to be peculiar to that particular mineral. Happening to mention this subject to the Hon. Mr. Cavendish, when I was in London, in the spring of the year 1772, he said that he did not imagine but that other kinds of pyrites, or the metals, might answer as well, and that probably the red appearance of the mixture depended upon the spirit of nitre only. This encouraged me to attend to the subject ; and having no pyrites, I began with the solution of the different metals in spirit of nitre, and catching the air which was generated in the solution, I presently found what I wanted, and a good deal more.

Beginning with the solution of brass, on the 4th of June 1772, I first found this remarkable species of air, only one effect of which was casually observed by Dr. Hales ; and he gave so little attention to it, and it has been so much unnoticed since his time, that, as far as I know, no name has been given to it. I therefore found myself, contrary to my first resolution, under an absolute necessity of giving a name to this kind of air myself. When I first began to speak and write of it to my friends, I happened to distinguish it by the name of *nitrous air*, because I had procured it by means of spirit of nitre only.

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I have found that this kind of air is readily procured from iron, copper, brass, tin, silver, quicksilver, bismuth, and nickel, by the nitrous acid only; and from gold and the regulus of antimony by *aqua regia*. The circumstances attending the solution of each of these metals are various, but hardly worth mentioning, in treating of the properties of the air which they yield; which, from what metal soever it is extracted, has, as far as I have been able to observe, the very same properties.

Nitrous air is procured from all the proper metals by spirit of nitre, except lead, and from all the semi-metals that I have tried, except zinc. For this purpose I have used bismuth and nickel, with spirit of nitre only, and regulus of antimony and platina, with *aqua regia*.

I did not endeavour to ascertain the exact quantity of nitrous air produced from given quantities of all the metals which yield it; but the few observations which I first made for this purpose I shall recite in this place:

dwt. gr.

6	0	of silver yielded	$17\frac{1}{2}$	ounce measures.
5	19	of quicksilver	$4\frac{1}{2}$	
1	$2\frac{1}{2}$	of copper	$14\frac{1}{2}$	
2	0	of brass	21	
0	20	of iron	16	
1	5	of bismuth	6	
0	12	of nickel	4	

Having,

Having, at another time, dissolved silver, copper, and iron, in equal quantities of spirit of nitre diluted with water, the quantities of nitrous air produced from them were in the following proportion; from iron 8, from copper $6\frac{1}{2}$, from silver 6. In about the same proportion also it was necessary to mix water with the spirit of nitre in each case, in order to make it dissolve these metals with equal rapidity, silver requiring the least water, and iron the most.

That iron contains more phlogiston than copper, is probable from the much greater quantity of nitrous air that it yields. At one time I found that two penny-weights of iron dissolved in spirit of nitre, diluted with rain-water, yielded forty five ounce measures of nitrous air, when the same quantity of pure copper, treated in the same manner, yielded only sixteen ounce measures.

I have observed that I got little or no air by dissolving *lead* in spirit of nitre. I afterwards, however, made another attempt of this kind, and with a little better success. I poured smoking spirit of nitre into a phial with a ground-stopper and tube, containing one ounce measure and a half, filled with small leaden shot, so as to leave no common air at all, either in the phial or in the tube; and I placed it so as to receive the air that might come from it in water,

After waiting an hour, in which little or no air was produced, I applied the flame of a candle,

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though not very near to it, and in these circumstances I got about an ounce measure of air; but upon some water rushing into the phial, while the candle was withdrawn, air was produced very plentifully. I collected, in all, about a quarter of a pint, and might probably have got much more; but that the salt formed by the solution of the lead had so nearly closed up the tube, that I thought proper to discontinue the process. The air, both of the first and of the last produce, was of the same quality, and so far nitrous, that two measures of common air, and one of this, occupied the space of two measures only; excepting that the very first and very last produce, mixed with common air, took up a little more room than that which I got in the middle of the process. When the air was produced very fast, it was exceedingly turbid, as if it had been filled with a white powder.

I have generally found it most convenient to get nitrous air from *copper*, on account of the pretty equable solution of that metal in the nitrous acid. If *iron* be made use of, the process is much more difficult, the increase of heat, and other circumstances, making a very great difference in the rapidity of the solution; so that very often, when the effervescence is very moderate at the beginning, it will be so violent after a short time, that the greatest part of the acid will be thrown out of the phial,
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and consequently the effect of it will be lost. This difficulty, however, only attends the pouring of a diluted spirit of nitre upon a quantity of nails, or other small pieces of iron; in order to effect a complete saturation of the acid that is made use of at one time, which I have found to be the most convenient upon the whole. If thicker pieces of iron be put to the acid, by which means the quantity of surface exposed to its action is not considerable, the produce of air may be made pretty regular; but I have not, upon the whole, found this method so convenient as the other.

Having sometimes, however, procured nitrous air from iron by this process, I have noted some circumstances attending this solution, which, because they are a little remarkable, I shall here recite. When I put a thick piece of iron into a quantity of very strong spirit of nitre, it was not at all affected by it: but by the application of a boiling heat it yielded nitrous air, about ten times the bulk of the acid. When a quantity of water was poured upon the spirit of nitre and iron, it became of a beautiful green or blue colour, and no motion was perceived in it for about a minute, when it burst out all at once into the most violent effervescence imaginable, and a prodigious quantity of nitrous air was instantly produced. It will be evident from subsequent experiments, that a certain proportion of *water* is ne-

cessary to the constitution of nitrous air, and therefore the diluted acid is more proper for this purpose.

Mr. Delaval was so obliging as to inform me that all *astringent vegetables*, as *galls*, the *peruvian bark*, and *green tea*, dissolve with peculiar rapidity in the nitrous acid; in a manner not unlike the solution of several of the metals in the same acid; and that a great quantity of air is generated in the process. I immediately made the experiment with *galls*, and was really surprized at the effect. The solution was, indeed, astonishingly rapid; but the quantity of air produced by it was not, seemingly, greater than would have been yielded by the same bulk of any other vegetable substance, dissolved in the same acid, with more heat. The air was also of the same quality with that which is yielded by most vegetable substances. In this case, more than half of it was fixed air, making lime water turbid, and the residuum was so far nitrous, that two measures of common air and one of this, occupied the space of two measures and a half.

SECTION II.

Of nitrous Air from Vapour of Spirit of Nitre and Water.

I WAS no sooner in possession of *nitrous vapour*, than I saw opened to me an entire new field for experiments, by means of a rapid solution of bismuth in spirit of nitre, of which a fuller account will be given under the article of *nitrous acid*.

Three methods presently occurred to me of applying this nitrous vapour, in order to form combinations with other substances, by which means only its proper nature, and peculiar powers, could be discovered. One was, to put the substance into a clean phial, and then to throw a stream of the vapour upon it. Another was, first to fill the phial with the vapour, by which method the quantity of it might be, in some measure, ascertained, and then to introduce the substance to it at the mouth of the phial. Lastly, if the substance was fluid, I could plunge the tube, through which the vapour was transmitted, as deep as I pleased into it, and thereby diffuse the vapour through the whole body of it. The second of these methods was the first I had recourse

recourse to, though soon afterwards I applied the first, and not long after that the third. And as I could not well produce this acid vapour at all without generating enough to fill a great number of phials, I generally placed six, eight, or ten of them in a row, filling them with the vapour one after another, and sometimes supplying them all several times in the course of one process.

The first experiment that I made with water, was to pour a small quantity of it into a phial filled with this vapour; when, shaking it about, it became, as would easily be supposed, genuine spirit of nitre; but it was weak and colourless.

After this, I threw a stream of the vapour upon a small quantity of distilled water, in a large phial, shaking it now and then, to promote the absorption of the vapour; when I observed that the water presently became warm, then began to sparkle very much, air issuing from all parts of it very copiously; and after this it assumed a light blue colour; in which stage of the process, it was, I doubt not, the very same thing that Mr. Woulfe had found by impregnating water with the superabundant nitrous vapour, in his method of distilling spirit of nitre. But, whereas he says his blue liquor continued blue, I found that mine presently lost its colour on being exposed to the open air, emitting a copious red fume,

Finding

Finding that, in this manner of impregnating water, I soon gained the point of saturation, by as much of the vapour escaping as I could readily throw into it, I contrived to impregnate the water more effectually, in the following manner. I got a vessel, *b*, fig. 2, Pl. V. in the form of a phial with a ground stopper, and two holes in the bottom; which, however, was to be placed uppermost when it was used. To one of these holes was fitted, by grinding, a glass syphon, one end of which was fitted in the same manner to the long phial in which the solution of the metal for the production of the vapour was made, while the other end of it went to the bottom of the vessel above mentioned, and which contained the water; so that whatever vapour was brought into the vessel by it, must necessarily pass through the whole body of the water; and to the other hole in this vessel there was fitted, by grinding also, the end of a bent tube, which conveyed the superfluous air, or vapour, into a common recipient. But sometimes I had several of these vessels connected together, as represented, fig. 3, so that the air and vapour discharged from the first of them must necessarily pass through the water in the next, and that which was discharged in this must pass through the water in the following, &c.

Making the experiment in this more accurate manner, so that the water had an opportunity of becoming thoroughly impregnated, I made the following observations. The water, after becoming warm, began, as before, to sparkle, and emit air; after which it became *blue*, still continuing to give air in much greater plenty than before. After this the water became *green*, about which time the emission of air ceased; and lastly, after the green colour had deepened very much, so as to appear almost black, when viewed in the same direction with the light that fell upon it, a *yellowish tinge* was perceived to be diffused through the green colour; and this was the last state to which I could bring the water by this impregnation.

I also observed that, about the time that the water in the first of these vessels became blue, that in the next began to sparkle; and when the water in the first turned green, which was probably effected in no other way than by the mixture of the *yellow* (which distinctly appeared afterwards) with the preceding blue, the water in the next vessel became blue, and that in the following to sparkle, &c.

One of the most extraordinary circumstances in this whole process, is the production of *air* from the water in the two first stages of it, viz. while it is transparent, and while it is blue, before it becomes

comes green. At first I concluded that this was phlogisticated air; this kind of air having been the produce of a similar process for the impregnation of *oil* with the nitrous vapour. But having filled a phial with this water, at the time that it was discharging air most copiously, and having placed it inverted in a basin of the same, I presently got a considerable quantity of it, and found it to be all pure nitrous air, possessed of the peculiar properties of that kind of air in as great a degree as any whatever, and that it contained no portion of fixed air.

The quantity of nitrous air produced in this manner is very extraordinary. When I filled a phial with the water in the state of emitting air, and inverted it in a basin of water, it presently almost filled it, expelling the water. But when I filled a phial with a ground stopper and tube with the water, and caught all the air that came from it, with and without heat, I got at one time more than ten times the bulk of the water, all pure nitrous air.

This will appear the more extraordinary, if it be considered, that water cannot be made to imbibe more than about one tenth of its bulk of nitrous air. The production of it in this case, therefore, is quite *another thing*, and must have a different *cause*; though, had the quantity of it been small, it might

have been imagined, that the nitrous air from the bismuth having impregnated the water, as, in some degree, it necessarily must, this nitrous air might have come from that solution.

So great is this discharge of nitrous air, that if the impregnated water be left to itself, it will continue to emit air for a day or two; so that it is not improbable, but that it may, from first to last, yield fifteen or twenty times its bulk. On this account, if this water be confined in thin phials, it will endanger the breaking of them; and the ground stoppers of strong phials have often been thrown out by it with great violence.

SECTION III.

Of the increased Produce of nitrous Air by previously converting the Acid into Vapour.

HAVING observed the remarkable production of nitrous air from water impregnated with nitrous vapour, in the following experiment I more accurately compared the quantity of nitrous air produced by pouring the impregnated water upon copper, with the quantity produced by an equal quantity of spirit of nitre and copper, without impregnating water with the vapour that the acid would have yielded.

Having dissolved a quantity of bismuth in a given quantity of spirit of nitre, and having made the vapour which was raised by the solution pass through a quantity of water, I poured this water on some clippings of copper, in a phial with a ground stopper and tube, and found that it yielded one sixteenth more nitrous air than the same quantity of nitrous acid diluted with water, and applied to the copper in the same manner, would yield; the heat of a candle being applied in both these cases till no more air could be procured. No allowance also was

made for a considerable quantity of red vapour which was lost in decanting the water, or for that which remained in the large phial in which the solution was made, or for the acid that was united with the bismuth in the solution. The air yielded by the impregnated water and copper was thirteen ounce measures, and the solution of the bismuth used in this experiment being diluted with water, and then poured upon the copper, yielded six ounce measures and a half, which alone is more than half as much as the original quantity of the acid yielded. Upon the whole, therefore, spirit of nitre, used in this manner, may be made to yield, by means of copper, one half more nitrous air than can be procured by it when applied in the usual way.

For my greater satisfaction, I also repeated an experiment similar to the former, with water impregnated with nitrous vapour, in the process for making dephlogisticated air from spirit of nitre and red lead, and the result was as follows. Having put six penny weights of strong spirit of nitre upon a quantity of red lead, and heating the mixture in a gun barrel, I made all the air, together with the redundant acid, pass through a quantity of water; and found that the water, poured upon copper, would have yielded fourteen ounce measures of nitrous air, (a part of the water having produced nitrous air in that proportion to the whole) but the same quantity
of

of the acid, even with the assistance of heat, yielded only about eleven ounce measures and a half.

I also mixed with three ounces of red lead as much spirit of nitre as occupied the space of eight pennyweights of water, when the produce was forty ounce measures of air, of which about five ounce measures was fixed air. The water through which it had passed in the vessel No. 2, Pl. V. after making all proper allowances, and using a variety of precautions in applying it to the copper, too minute to be mentioned here, I judged to produce in all twenty four ounce measures of nitrous air, which I found to be clearly more than the original quantity of this acid would have yielded.

The above-mentioned experiments were made before I had much suspicion of the great difference in the produce of nitrous air occasioned by the application of *heat*, which is sometimes very considerable, and by no means in the same proportion in all cases; some kinds of the acid yielding almost the whole produce without external heat, and other kinds hardly more than one half. I therefore thought it necessary to go over this process once more with a view to this circumstance, and the result was still the same as before, the water through which the generated air had passed producing more nitrous air than the whole quantity

tity of the acid employed in the experiment would have done.

The quantity of acid which I used at this time occupied the space of four pennyweights of water, and when applied to copper I could not, with any application of heat, make it yield more than twelve ounce measures and a half. But when the same quantity of this acid had been mixed with red lead, which was afterwards put into a gun barrel, and had been made to yield all the air that could be extracted from it, one seventh part of the water through which the air had passed produced two ounce measures of nitrous air; so that the whole quantity would have been fourteen ounce measures; and this was after the water had been decanted first from the vessel represented fig. 2, into another phial, and some time afterwards, from that into the small phial containing the copper. And it should be considered, that after this process (if it be continued till the water begin to emit air, a circumstance of which an account will be given hereafter) it is so exceedingly volatile, that it is not possible to pour the water from one vessel to another without the discharge of very copious red fumes, in which a good deal of the acid must be lost. There must also be some loss of that nitrous air which is emitted by the water itself; and I doubt not that the
the

the increase in the produce of nitrous air in these experiments is from this source, viz. that which is supplied from the water, in consequence of the impregnation with nitrous vapour. Whereas when the acid is much dephlogisticated, a great part of it becomes combined with the menstruum, and therefore has no effect in producing nitrous air.

I hardly remember any thing, in the whole course of my experimenting, that appeared more extraordinary than this. It seemed as if there was an increase, instead of any loss of acid, after part of it must have been employed in forming the air, and part also had been necessarily lost in the course of the experiment.

I consulted several of my chemical friends upon this subject; but they were all of them as much at a loss to account for the fact as myself.

That the giving of nitrous air depends upon phlogiston, is evident from the phenomena which attend the solution of iron in phlogisticated and dephlogisticated acids. Pouring a small quantity of phlogisticated nitrous acid into a large quantity of water, which had iron wire in it, it presently became of a dark colour; but this was soon precipitated, and the solution assumed a lighter colour. I then poured off the solution, which was of a slight brown colour, and pouring into it more phlogisticated

cated nitrous acid, it immediately became of a very dark colour, and emitted air copiously.

On examination it appeared to be strong nitrous air. After the emission of this air, the dark colour disappeared. These phenomena, therefore, exactly resembled those of a solution of green vitriol, which assumes a dark colour by imbibing nitrous air, and becomes clear again by the expulsion of it. The dark green spirit of nitre had the same effect as the brown phlogisticated acid, but the dephlogisticated nitrous acid had no such effect.

It is easy to make a pretty strong solution of iron in dephlogisticated nitrous acid that shall be green and give no air, if it be kept very cold during the process. But if phlogisticated nitrous acid be poured into the solution, it presently becomes very black, and emits air. This blackness will sometimes, if the nitrous acid be very volatile, go off almost immediately; but in all cases it will do so *in time*, and leave the liquor like water, or with a slight tinge of yellow; owing probably to part of the ochre having imbibed pure air, and thereby tending to become red.

Nitrous air also admitted to a green solution of iron in nitrous acid immediately turns it black, just as it does a solution of green vitriol.

Phlo-

Phlogificated nitrous acid dropped into a solution of green vitriol also makes it black. The green solution of iron in spirit of nitre, yields very little air by heat, and this is not nitrous air. When charcoal was put into it, and heated, it also gave little or no air.

SECTION IV.

Of the Production of nitrous Air by Means of phlogificated nitrous Acid.

THE only method that I have used to measure the strength of different kinds of nitrous acid, has been to find the quantity of nitrous air that a given quantity of the acid would yield, when diluted with equal quantities of water, from the same quantity of copper. It is necessary that these circumstances be pretty rigorously attended to; for otherwise considerable mistakes will be made. For in different circumstances the produce of air from equal quantities of the same acid will be considerably different. I shall here subjoin a few of my observations of this kind, that the reader may be apprized of them, and also of the importance of attending to other differences of a similar nature.

In

In a small phial, and with a brisk effervescence, the quantity of four penny-weights of water of a strong spirit of nitre produced sixteen ounce measures of nitrous air; whereas, in a large phial, diluted with more water, and consequently with a less effervescence, the same quantity of the same acid yielded only fourteen ounce measures of air. Also the quantity of copper (which was such cuttings as the braziers commonly make) in the small phial was about half as much as that in the large one. With the same quantity of spirit of nitre in the large phial, and with the application of the heat of a candle, I got fifteen ounce measures of air. At another time the same quantity of the acid without heat has not yielded much more than twelve ounce measures.

I have frequently observed that, unless the quantity of acid was sufficient to produce a *brisk effervescence*, the produce of air has been greatly deficient, the briskness of the effervescence occasioning a considerable *heat*, which is always favourable to the solution of metals. But the application of equal degrees of heat will not make the produce of air equal, unless other circumstances be attended to. Whenever I have compared the strength of acids in this manner I have scrupulously attended, as far as I could, to all these circumstances.

Having

Having procured nitrous acid in the several states above-mentioned, viz. the original pale coloured acid, that out of which the colour had been expelled by heat, that which had been distilled again from fresh nitre, and that which had been phlogisticated by heat in close vessels, I tried the strength of them all by the solution of copper, measuring the quantity of nitrous air that equal bulks of them (all other circumstances being the same) produced, and observed that a quantity of each occupying the space of two pennyweights eighteen grains of water yielded as follows, viz.

Ounce Measures.

The original pale coloured acid,	14
The colourless, - - -	11
That redistilled from nitre,	11
That coloured by heat, - -	11

This highly phlogisticated acid hissed very much when mixed with water. The produce of air was more or less accelerated during the course of the solution in all of them, but most of all when I used the pale coloured acid. I must observe that, in making this colourless acid, I used more heat than was necessary, and therefore weakened it too much, though it is certainly impossible to expel the colouring phlogiston without expelling, at the same time, the acid to which it is attached. It is something remarkable, that the phlogiston, *in this*

par-

particular state, should attach itself wholly to one part of the acid only, though mixed with the rest of the acid, combined also with phlogiston, but in a different state. These experiments, however, sufficiently demonstrate this to be the case.

It is something remarkable, that though a great quantity of nitrous air is produced by the solution of copper in a diluted nitrous acid, no air at all is procured by a solution of the same metal in the strong acid. There is not even any appearance of air being formed, and afterwards absorbed by the acid, as in the similar solution of mercury.

Having saturated a quantity of strong spirit of nitre with copper, of which it dissolves but a small quantity, I distilled it in a green glass retort. The first part of the acid that came over was orange coloured, from being of a deep green; but the last was quite transparent and weak. No air, that I could perceive, was produced, but a tubulated receiver being made use of, a small quantity could not be discovered. There was not water enough to form nitrous air.

SECTION V.

Of Air from Gunpowder.

BEING desirous of knowing what kind of air was produced by the explosion of gunpowder, I, for that purpose, mixed equal quantities of sulphur and salt-petre, both finely pounded, and put them into a tall glass vessel. The production of air was very rapid and copious, and so highly nitrous, that two measures of common air, and one of this, occupied the space of two measures and a quarter. Since the produce of air from spirit of nitre and charcoal is the very same with this, viz. nitrous air, it cannot be doubted but that nitrous air is also produced in the explosion of gunpowder, which is composed of those ingredients; the spirit of nitre not being destroyed, or so far decomposed as that its acid nature is lost, but only entering into the composition of this species of air.

Having got nitrous air from a mixture of salt-petre and sulphur, and also from spirit of nitre and charcoal, I concluded that nitrous air must be produced in the firing of gunpowder; and it favours this supposition, that when I fired gunpowder

in common air, the air was in part phlogisticated by it. It is possible, however, that when the heat is applied very *suddenly*, the proper *earth* of the charcoal, and also that of the nitre itself may, in part, unite with the nitrous acid, and thereby compose a better kind of air than was produced in those experiments, in which the process was slow, so that the spirit of nitre had an opportunity of saturating itself with the *phlogiston* of the substances mixed with it, without touching the pure *earth*, and therefore the produce was nitrous air only.

I have been led to entertain this suspicion in consequence of being invited by Mr. Woulfe to examine the air that is produced in making *clyffus of nitre*, both with sulphur and with charcoal; when, in both the cases, I own, the air that was produced appeared to be considerably better than, from the materials, and the manner of making the experiments, I should have imagined it could have been. There was, indeed, a considerable quantity of common air in every thing belonging to the apparatus, which was not constructed with any view to the produce of air; but the process was continued so long, and the quantity of air produced was so great, that I do not, in my own mind, make much allowance for that circumstance.

It appeared that the air produced from the *clyffus* made with *sulphur*, contained one twelfth of fixed air,

air, making lime water turbid, and the remainder was phlogisticated air, neither affecting common air, nor being affected by nitrous air, and extinguishing a candle. And the air that was produced in the process with *charcoal*, contained no more than one twentieth of fixed air, and the remainder, though it extinguished a candle, was so little phlogisticated, that two measures of it and one of nitrous air occupied the space of two measures and a quarter.

P A R T II.

OF THE PROPERTIES OF NITROUS AIR.

SECTION I.

Of nitrous Air as the Test of the Purity of respirable Air.

ONE of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. The *smell* of it, also, is very strong, and remarkable, but very much resembling that of smoking spirit of nitre.

The diminution of a mixture of this and common air is not an equal diminution of both the kinds, which is all that Dr. Hales supposed he had observed, but of about one fourth of the common air, and as much
of

of the nitrous air as is necessary to produce that effect; which, as I have found by many trials, is about one third as much as the original quantity of common air. For if one measure of nitrous air be put to two measures of common air, in a few minutes (by which time the effervescence will be over, and the mixture will have recovered its transparency) there will want about one ninth of the original two measures; and if both the kinds of air be very pure, the diminution will still go on slowly, till in a day or two, there will remain only one fifth of the original quantity of common air. This farther diminution, by long standing, I had not observed at the time of my first publication on this subject.

I hardly know any experiment that is more adapted to amaze and surprize than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is considerably diminished by it. If, after this full saturation of common air with nitrous air, more nitrous air be put to it, it makes an addition equal to its own bulk, without producing the least redness, or any other visible effect.

In order to judge whether the water contributed to the diminution of this mixture of nitrous and

common air, I made the whole process several times in quicksilver, using one third of nitrous, and two thirds of common air, as before. In this case the redness continued a very long time, and the diminution was not so great as when the mixture had been made in water, there remaining one seventh more than the original quantity of common air.

This mixture stood all night upon the quicksilver; and the next morning I observed that it was no farther diminished upon the admission of water to it, nor by pouring it several times through the water, and letting it stand in water two days.

Another mixture, which had stood about six hours on the quicksilver, was diminished a little more upon the admission of water, but was never less than the original quantity of common air. In another case, however, in which the mixture had stood but a very short time in quicksilver, the farther diminution, which took place upon the admission of water, was much more considerable; so that the diminution, upon the whole, was very nearly as great as if the process had been entirely in water.

It is evident from these experiments, that the diminution is in part owing to the absorption by the water; but that when the mixture is kept a long time, in a situation in which there is no water to absorb any part of it, it acquires a constitution,

stitution, by which it is afterwards incapable of being absorbed by water, or rather, there is an addition to the quantity of air by nitrous air produced by the solution of the quicksilver.

It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, or *air fit for respiration*; and, as far as I can judge from a great number of observations, is at least very nearly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by putting mice, or any other animals, to breathe in it.

This was a most agreeable discovery to me, as I hope it may be an useful one to the public; especially as, from this time, I had no occasion for so large a stock of mice as I had been used to keep for the purpose of these experiments, using them only in those which required to be very decisive; and in these cases I have seldom failed to know beforehand in what manner they would be affected.

It is also remarkable that, on whatever account air is unfit for respiration, this same test is equally applicable. Thus there is not the least effervescence between nitrous and fixed air, or inflammable air, or any species of diminished air. Also the

degree of diminution being from nothing at all to more than one fourth of the whole of any quantity of air, we are, by this means, in possession of a prodigiously large *scale*, by which we may distinguish very small degrees of difference in the goodness of air.

I have not attended much to this circumstance, having used this test chiefly for greater differences; but, if I did not deceive myself, I have perceived a real difference in the air of my study, after a few persons have been with me in it, and the air on the out side of the house.

By means of this test I was able to determine what I was before in doubt about, viz. the *kind* as well as the *degree* of injury done to air by candles burning in it. I could not tell with certainty, by means of mice, whether it was at all injured with respect to respiration; and yet if nitrous air may be depended upon for furnishing an accurate test, it must be rather more than one third worse than common air, and have been diminished by the same general cause of the other diminutions of air. For when, after many trials, I put one measure of thoroughly putrid and highly noxious air, into the same vessel with two measures of good wholesome air, and into another vessel an equal quantity, viz. three measures of air in which a candle had burned out; and then put equal quantities of nitrous

trous air to each of them, the latter was diminished rather more than the former.

It agrees with this observation, that air in which a candle has *burned* is farther diminished both by putrefaction, and a mixture of iron filings and sulphur; and, I therefore take it for granted, by every other cause of the diminution of air. It is probable, therefore, that this air is air so far loaded with phlogiston, as to be able to extinguish a candle, which it may do long before it is fully saturated.

I would observe, that it is not peculiar to nitrous air to be a test of the fitness of air for respiration. Any other process by which air is diminished, and made noxious, answers the same purpose. Liver of sulphur for instance, the calcination of metals, or a mixture of iron filings and brimstone will do just the same thing; but the application of them is not so easy, or elegant, and the effect is not so soon perceived. In fact, it is *phlogiston* that is the test. If the air be so loaded with this principle that it can take no more, which is seen by its not being diminished in any of the processes above-mentioned, it is noxious; and it is wholesome in proportion to the quantity of phlogiston that it is able to take.

This, I have no doubt, is the true theory of the diminution of common air by nitrous air, the red-

ness of the appearance being nothing more than the usual colour of the fumes of spirit of nitre, which is now disengaged from the superabundant phlogiston with which it was combined in the nitrous air, and ready to form another union with any thing that is at hand, and capable of it.

I found, very unexpectedly, that a considerable difference would be made in the dimensions of the mixture of air by a circumstance in the *manner of mixing* them that one would not readily suspect, and I was not at first able to account for it. My usual method, as I have observed in the Introduction, has been to mix equal measures of nitrous and common air in a low jar, and then to transfer the air into a graduated tube, three or four feet long. What I observed is, that I could make a difference of five hundred parts of a measure by making the air run up the long tube quickly or slowly. The more slowly it ascended, the less space it occupied. To ascertain whether it depended merely upon the two kinds of air being so much longer together in the wider vessel, or in the funnel through which it was poured into the tube, I made the mixtures over night, and transferred them into the graduated tube the next morning; but I still found the same difference, depending upon the circumstance above-mentioned. It has been observed by Mr. Cavendish, that *agitation*

tation brings a mixture of common air and nitrous air into much less compass than a mixture of them without agitation. The difference is indeed very great, and therefore should always be mentioned. But in this case there was no proper agitation.

The fact above mentioned, I now conclude arose from what remained of the nitrous air, not decomposed in the mixture, being diminished by passing through so much space of water, which is more exposed to its influence in a slow than in a quick passage. But I own I should not have suspected that nitrous air would have been diminished so very much by being simply poured from one vessel of water into another, if I had not observed it in the following manner.

Having mixed a quantity of air, which I knew to be thoroughly phlogisticated by the putrefaction of fishes, with an equal quantity of nitrous air, I transferred the mixture into my graduated tube; when, instead of occupying two whole measures, as I had expected, they only occupied 1.95 measures. Suspecting that the five hundred parts of a measure which had disappeared had been absorbed by the water, I poured the air back again into the wide jar; and transferring it once more into the graduated tube, found it to be only 1.8 measures; and pouring it about ten times backwards and forwards,

wards, without any unnecessary agitation, it was reduced to 1.6. Having stood in water all night, I measured it again the next morning, when I found it to be 1.5; and by measuring three times more it was reduced to 1.4.

I then poured two measures of nitrous air only from the wide jar into the graduated tube, and found that it was diminished even in a greater proportion than the former mixture.

In applying the test of nitrous air, I have lately preferred equal measures of nitrous and of common air, or of any air which may be conjectured *a priori* to be nearly in the state of common air, in order that there might be phlogiston enough to saturate it entirely; and if the remaining nitrous air was not affected by water, this method would be perfectly unexceptionable; and with due precaution, it is not liable to much objection. But the most accurate method would be to use no more nitrous air than the air to be examined is able completely to decompose. But then it cannot be known before hand how much this is. Perhaps, in order to guard against the inconvenience above mentioned, it might be most adviseable, in common cases, that is, when the air to be examined is about the standard of common air, to use something less than an equal quantity of nitrous air, but more than one half,
which

which was the quantity that I first confined myself to.

I rather suspect that when nitrous air is mixed with common air, in a greater proportion than is requisite to the complete saturation of the common air with phlogiston, the superfluous nitrous air is more disposed to be absorbed by water than pure nitrous air. It appears, however, that, in no great length of time, such mixtures are brought to the same dimensions as if only half the quantity of nitrous air had been mixed with the common air. This, I think, may be inferred from an experiment which I made to try the difference between *old* and *fresh made* nitrous air, both having been made in the same manner, and, I believe, having been originally of equal strength.

October 25, 1777, I mixed equal quantities of the same common air with equal quantities of the old and fresh made nitrous air. What space they occupied at that time, and in several subsequent periods, is represented at one view, as follows :

	With the old nitrous air.	With the new.
Oct. 27, 1777,	1.22	1.05
Nov. 10,	1.07	0.93
24,	0.96	0.86
Feb. 2, 1778,	0.84	0.8

The last is one fifth less than the original bulk of the common air, and consequently very near to the

the utmost limit of the diminution of common air by any proper phlogistic process. An accident prevented my observing this progress any farther.

SECTION II.

Of the Impregnation of Water with nitrous Air.

HAVING, among other kinds of air, exposed a quantity of nitrous air to water, out of which all air had been well boiled, in the experiment to which I may more than once refer (as having been the occasion of several new and important observations) I found that nineteen twentieths of the whole was absorbed. Perceiving, to my great surprize, that so very great a proportion of this kind of air was miscible with water, I immediately began to agitate a considerable quantity of it, in a jar standing in a trough of the same kind of water; and, with about four times as much agitation as fixed air requires, it was so far absorbed by the water, that only about one fifth remained. This remainder

mainder extinguished flame, and was noxious to animals.

Afterwards I reduced a pretty large quantity of nitrous air to one eighth of its original bulk, and the remainder still retained much of its peculiar smell, and diminished common air a little. A mouse also died in it, but not so suddenly as it would have done in pure nitrous air. In this operation the peculiar smell of nitrous air is very manifest, the water being first impregnated with the air, and then transmitting it to the common atmosphere.

This experiment gave me the hint of impregnating water with nitrous air, in the manner in which I had before done it with fixed air; and I presently found that distilled water would imbibe about one tenth of its bulk of this kind of air, and that it acquired a remarkably astringent taste from it. The smell of water thus impregnated is at first peculiarly pungent. I did not chuse to swallow any of it, though, for any thing that I know, it may be perfectly innocent, and perhaps, in some cases, salutary.

This kind of air is retained very obstinately by water. In an exhausted receiver a quantity of water thus saturated emitted a whitish fume, such as sometimes issues from bubbles of this air when it is first generated,

generated, and also some air-bubbles ; but though it was suffered to stand a long time in this situation, it still retained its peculiar taste ; but when it had stood all night pretty near the fire, the water was become quite vapid, and had deposited a filmy kind of matter, of which I had often collected a considerable quantity from the trough in which jars containing this air had stood. This I suppose to be a precipitate of the metal, by the solution of which the nitrous air was generated. I have not given so much attention to it as to know, with certainty, in what circumstances this *deposit* is made, any more than I do the matter deposited from inflammable air above-mentioned ; for I cannot get it, at least in any considerable quantity, when I please ; whereas I have often found abundance of it, when I did not expect it at all.

The nitrous air with which I made the first impregnation of water was extracted from copper ; but when I made the impregnation with air from quicksilver, the water had the very same taste, though the matter deposited from it seemed to be of a different kind ; for it was whitish, whereas the other had a yellowish tinge. Except the first quantity of this impregnated water, I could never deprive any more that I made of its peculiar taste. I have even let some of it stand more than a week,

in phials with their mouths open, and sometimes very near the fire, without producing any alteration in it*.

In the beginning of May 1776, I saturated a quantity of distilled water with nitrous air produced from *bismuth*, and it happened to stand ten days, or a fortnight, in the phial in which the impregnation was made, the superfluous nitrous air lying upon the surface of it. Then, mindful of the caution suggested by Mr. Bewly, not to admit the common air to this nitrous air in contact with the water, I very carefully, and as quickly as possible, slipped a small funnel into the mouth of the phial, in the instant that I turned it upside down; and immediately I filled it up with some of the water in the basin in which it had been inverted, so that the nitrous air, in its escape mixed with the common air, and was decomposed, on the outside of the phial, and not within it. I have forgotten with what particular view I had made this impregnation, but I had no expectation of the result, till, observing it the day following, I found that it had deposited a considerable quantity of very *white matter*, and that the water did not retain the least sensible degree of acidity, not even turning the juice of turnsole red. This

* I have since found, that nitrous air has never failed to escape from the water, which has been impregnated with it, by long exposure to the open air.

experiment

experiment I have endeavoured to repeat, but always without success.

At one time, in order to determine whether the precipitate from water impregnated with nitrous air was different according to the *metal* made use of in procuring the air, I impregnated three quantities of distilled water with nitrous air, of which one was procured from *bismuth*, another from *copper*, and a third from *iron*, each in an eight ounce phial. In all these cases the water imbibed about one sixth of its bulk of this air; and when the impregnation was completed, I, as quickly as possible, and in the manner described in the last mentioned experiment, filled all the phials with water from their respective basons. But very little deposit was observed for a considerable time, and the water in all the phials turned the juice of turnsole red. This impregnation was made on the 28th of May, and the deposit having been made gradually, and as far as I could observe equally, the quantity of it was, in the beginning of October, pretty considerable; but still not more than half of what was deposited in the first mentioned experiment. In all the phials, also, the colour of the deposit was the same, viz. a *dark brown*. The water also in them all was still acid, but not, I think, in so great a degree as at first.

Imagining

Imagining that the difference might depend upon the *time* that the superfluous nitrous air had remained upon the surface of the water (during which I had never observed any deposit to be made) I let several of these impregnations remain a fortnight, and some more than a month, before I inverted the phials; but still the deposit was made as slowly as before, and was always of a brownish colour. In some cases this deposit was very considerable.

When I heated this water, or when I put it into an exhausted receiver, and thereby expelled from it all the air that I could, very little more deposit was made than there would have been if no air had been extracted from it. Also whether the phials containing the impregnated water were closely stopped, or left quite open, there was no difference with respect to the deposit.

I imagined that I should have procured a considerable quantity of this deposit by decomposing a large quantity of nitrous air, which I did by means of common air, in a small quantity of water. But though I repeated this process till the water was become exceedingly acid, it made no more deposit in a few days than would have been made from water simply impregnated with nitrous air. One phial of this water I put under an exhausted receiver; but though, by this means, a considerable quantity

of air was discharged from it, it made no more deposit than the rest.

Imagining that the *acid* which remained in this water might prevent the deposit from being made, especially as in the first experiment, in which the deposit was so considerable, the water did not retain any sensible acidity, I put a little caustic alkali to the impregnated water; but no visible effect followed from it. To prevent all acidity as much as possible, I did not always depend upon my address in applying the funnel, in the manner described above, but I let out the superfluous nitrous air in a trough of the same water that had been impregnated with it, so that it was impossible for it to be in the least affected by the decomposition of it with the common air. But still the result was not at all different from what it had been in the other cases in which this precaution had not been taken.

Mr. Bewly has very well observed, that that acidity of water impregnated with nitrous air which is *sensible to the taste*, is given to it by the decomposition of the nitrous air in contact with the impregnated water; but I have found, that a slight degree of acidity, not indeed sensible to the taste, but discoverable by the juice of turnsole, is always communicated to water by its impregnation with nitrous air. For if a phial be filled with water tinged blue with the juice of turnsole, and then the nitrous air
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be admitted to it, and agitated in it, in order to promote the impregnation, a change of colour will presently be perceived in the water. But rain-water so impregnated (the superfluous air being let out under water) retains so little acidity, as hardly to be discovered by mixing it with other water tinged blue.

I once imagined that nitrous air might possibly undergo some change in its constitution in consequence of its being imbibed by water; and for some time I always expelled a proportion of *fixed air* along with the nitrous, from water so impregnated; but by using the following precaution I discovered my mistake. I carefully pumped all the air out of a quantity of rain water, letting it stand twenty four hours in a very good vacuum, and then impregnated it with nitrous air; when, immediately expelling all that I could of it by the heat of boiling water, I found no part of it fixed air, but all pure nitrous air; though not more than one fourth of the quantity that had been imbibed by it.

I wish I could have given my reader more satisfaction with respect to this *deposit* made by nitrous air; but though I have given more attention to it than perhaps to any other subject relating to air, I have not hitherto succeeded to my wish. Perhaps I may be more fortunate hereafter. I have little doubt, however, but that this precipitate consists of the calx of the metal, by the dissolution of which

the nitrous air is procured, and the white colour of the first deposit from bismuth may arise from a less portion of phlogiston adhering to it than to the *brown* precipitates. But what I want is a method of making the precipitate *at pleasure*, that a quantity might be procured for a careful examination, and that the proportion of it, in a given quantity of air, might be ascertained.

SECTION III.

Of the Absorption of nitrous Air by Oils, Spirit of Wine and caustic Alkali.

THAT *water* would imbibe a certain portion of nitrous air, I discovered pretty early; but that *oils* would do it, and especially in such a prodigious quantity, and so very rapidly, as I afterwards found they do, I did not so much as suspect at the time of my last publication; and the experiments will shew that the decomposition is effected by means of the affinity which oils, and especially the essential oils, are known to have with the nitrous acid, or its base.

For

For it is evidently this part of the nitrous air that they imbibe.

Having seen some reason to suspect what would be the consequence of admitting nitrous air to oil of turpentine, from what I had observed in my impregnation of oils with the *nitrous vapour*, as will be seen hereafter, I filled a small glass jar with oil of turpentine, inverted in a basin of the same; and on expelling that fluid by filling it with nitrous air, I observed that, without any agitation, the nitrous air was diminished so fast, that in about six hours three fourths of it quite disappeared. What remained extinguished a candle, being, in all respects, the same with *phlogisticated air*, or that to which nitrous air is reduced by iron filings and brimstone, long agitation in water, and other processes.

When I *agitated* nitrous air in oil of turpentine, it was absorbed quite as readily as fixed air is absorbed by water; but the quantity of nitrous air that oil of turpentine will imbibe is vastly greater than the quantity of fixed air that water can be made to receive.

What is the *limit* in this case I cannot tell; but throwing away the residuum, which could not be imbibed by oil of turpentine (which was generally about one fourth of the whole, the same as in the process with iron filings and brimstone) I made a

quantity of this oil imbibe, at different times, eleven times its bulk of nitrous air, and with very great ease, even at the last; but not quite so readily as at the first.

During this process, the oil, from being transparent, presently became of a light orange colour, and then had a yellowish cast, and was a little glutinous; but towards the end of the process part of the oil became of a very deep orange, and, separating from the rest, sunk to the bottom of the vessel. It must have been the nitrous acid formed by the nitrous air, and the acidifying principle of pure air, contained perhaps in the oil of turpentine; and it would probably have decomposed more nitrous air, till the whole of it had been converted into this thick orange coloured mass; which is the same thing, as will be seen in its proper place, with this oil after it has been fully impregnated with *nitrous vapour*.

I endeavoured to expel air from the oil of turpentine which had imbibed such a quantity of nitrous air, but though I applied a considerable degree of heat, no air came from it.

Willing to know the *last state* to which the impregnation with nitrous air would bring oil of turpentine, I put a small quantity of it into a thin phial, ballancing it so that it would swim upright in water, and then introduced it into a large jar of
nitrous

nitrous air standing in water. It absorbed, in all, two thirds of it; at first very slowly, but afterwards more rapidly; the water rising more in one day than it had done in several days before, and the whole process lasted a week; after which part of the oil of turpentine was become orange coloured and thick, sinking to the bottom of the phial, but the change of colour was made at the surface. After about ten days I took it out of this jar, and put it into another jar of fresh nitrous air, when it began to absorb the air very fast, having imbibed about one fourth of it in one night.

Seeing no other appearance than the change of the oil of turpentine into this dark orange coloured mass, I at length discontinued the process, and exposed the substance I had procured to the open air, when it became gradually thicker, till, in a month or six weeks, it became almost as hard as glue. The inside of the jar in which this experiment was made was nearly covered with small specks of the same glutinous orange coloured matter; the impregnated oil having, no doubt, been exhaled, and having settled on the sides of the glass, where, the more limpid part being evaporated, the rest became of the consistence above mentioned.

I also made a quantity of oil of turpentine imbibe nitrous acid from nitrous air, by saturating

with it the common air in the phial in which it was contained. It presently became very hot; was first green, and then of an orange colour, and parts of it becoming very thick and glutinous, sunk to the bottom, exactly as the oil of turpentine which had imbibed the nitrous air in the preceding process, or the nitrous vapour. N. B. After I had made some progress in this operation, it went on very rapidly. For immediately after I had applied the bladder of nitrous air to the phial, it rushed into it, and all the nitrous air was decomposed in a few seconds. In this circumstance, also, there is a remarkable resemblance between the two processes; the decomposition of the nitrous air in both cases not having been effected so rapidly at the first, as some time afterwards.

Ether has the same power of absorbing nitrous air that oil of turpentine possesses. Having filled a phial with ether, and inverted it in a basin of the same, I introduced a quantity of nitrous air into it, in the same manner as I had done to the oil of turpentine; and presently found that, with a very little agitation, three fourths of it disappeared, and the remainder possessed no nitrous property.

Willing to see the whole effect of nitrous air upon ether, I introduced a small quantity of it into a large jar of nitrous air, in the same manner as I had done with oil of turpentine in the above-mentioned

mentioned experiment. For several days air kept bubbling out at the bottom of the jar (the effect of ether on all kinds of air, as I have observed, being to increase, and almost double the whole quantity of it) but after this time the air in the jar began to be diminished, and the water to rise in it, the phial containing the ether always swimming on its surface. But at the end of the process, which continued about three weeks, one third of the air in the jar remained. After this I perceived no alteration in its quantity: but, letting it remain a fortnight longer, I examined it, and found the ether very much diminished in quantity, though not changed in its appearance; but it did not evaporate on being exposed to the open air as ether does. What was most remarkable was, that the nitrous air had lost hardly any thing of its peculiar property of diminishing common air. But it may be supposed that there was not a quantity of ether sufficient to produce any considerable change in so large a quantity of nitrous air; and the reason why what remained of the ether, after this experiment, did not evaporate, might be, that the exhalation of the water within the jar had mixed with it, and diluted it very much.

Olive oil, likewise, imbibes nitrous air, but not rapidly, perhaps about half as fast as water imbibes fixed air without agitation; which makes very little

little difference in the case of *oil*, on account of its viscosity, and consequently its not being much divided by that operation. By long standing, a quantity of olive oil imbibed almost the whole of a small quantity of nitrous air.

Olive oil, by which a quantity of nitrous air had been confined in a phial several months, had absorbed almost the whole of it, and that part of the oil which was contiguous to the air was coagulated in lumps, as if it had been frozen, and remained a long time at the top of the oil. But afterwards, being loosened, I suppose, by the warmth of the weather, it all sunk to the bottom, as the ice of oil always does.

This property of diminishing nitrous air, is not peculiar to oils. It is likewise found in *caustic alkali*, though not in the same degree. Imagining that the preceding oils seized upon the acid of nitrous air, and thereby decomposed it, I thought that alkalies, having the strongest affinity with acids, caustic alkaline liquors, fixed and volatile, must have the same effect; and the experiments seemed to verify my conjecture. Having put a quantity of nitrous air to a phial of caustic fixed alkali, immersed in a basin of the same, I observed that, in the space of three days, and without agitation, so much of it had been absorbed, that not more than one sixth of the quantity remained, and after

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six days about one twelfth part of it only was left.

An equal quantity of *volatile alkali*, in similar circumstances, imbibed, at the same time, but little of the nitrous air. But at another time, after waiting about a week, I observed that a quantity of it had absorbed about one third of a small quantity of nitrous air that had rested upon its surface.

At another time I observed that a quantity of fixed alkali absorbed almost the whole of about one fourth of its bulk of nitrous air; for the remainder could not be more than one twentieth part. But when a phial was quite filled with nitrous air, and placed with its mouth in a basin of the same fluid, the absorption went on very slowly.

When, by means of agitation, I had made a quantity of fixed caustic alkali imbibe its bulk of nitrous air, I observed that the colour of it was not in the least sensibly changed; also it had no more effect upon iron than it had before this process.

Caustic alkali had no sensible effect either on common or inflammable air, though only a small quantity of these kinds of air was kept in contact with a large quantity of this liquor about a week.

At the same time that I first exposed nitrous air to oil of turpentine, I, in the same manner, brought

brought it into contact with *spirit of wine*; and at that time the absorption, without agitation, seemed to be almost as considerable as that with the oil of turpentine. But though this fluid imbibed the nitrous air very fast at the first, it was soon saturated, which is not the case with oil of turpentine. By repeating the process several times, I made a quantity of spirit of wine imbibe its bulk of nitrous air. But after this it received more air with great difficulty; and though I did not urge it to the utmost, I do not think that it would have taken much more. No change was produced in the appearance of the spirit of wine, it being as transparent as at first; and, what I thought a little remarkable, it did not affect the juice of turnsole in any other manner than spirit of wine always does. The application of *heat* to the spirit of wine thus impregnated did not expel any air from it, any more than it had done from the oil of turpentine impregnated in the same manner.

In order to compare the absorption of nitrous air by spirit of wine and by oil of turpentine, I filled two cylindrical glass vessels, nine inches in length, one with oil of turpentine, and the other with spirit of wine, inverting them in basons of the same. Then, expelling the liquors, I filled them both completely with nitrous air, and observed that in less than a day the oil of turpentine had absorbed three fourths of its air, while the spirit of
wine

wine had not risen in the jar more than three quarters of an inch, and it never advanced any higher.

SECTION IV.

Of the Phenomena attending the Absorption of nitrous Air by acid Liquors.

AS nitrous air is liable to be decomposed by any substance that has a near affinity either with its phlogiston, or with any other of its constituent parts, it was natural to think of trying the effect of the several *acids*, which are known to have a considerable affinity with phlogiston. Accordingly, about the same time that I made the experiments described in the last section, I conveyed a quantity of nitrous air into phials previously filled with the *vitriolic*, *nitrous* and *marine acids*; and it presently appeared that all of them got phlogiston from this air; but the quantity of it which the nitrous acid decomposed, the quickness of the process, and the effect of it upon the nitrous acid itself,

itself, were appearances that I viewed with astonishment, having had no expectation of any such result; and several good chemists of my acquaintance have expressed no less surprize at them than myself, though these facts will appear less extraordinary, when it is considered how very strong is the affinity between this acid and phlogiston. This, however, is perhaps a more evident proof of the peculiar strength of this affinity than any other fact that chemistry has hitherto furnished.

Having, in all the other cases, had occasion to agitate this species of air in the fluids which I expected to absorb it; the moment that I introduced *this* air to the nitrous acid, I was, as usual, beginning to agitate it; but with the least motion the absorption was almost instantaneous, being nearly as quick as the absorption of acid or alkaline air by water; and the quantity of nitrous air that a very small proportion of this acid is able to decompose, and to appearance absorb, almost exceeds belief.

Finding this absorption so very rapid, I had no occasion to introduce the air into phials previously filled with nitrous acid, in the manner in which I had done with respect to other fluids, but only filled the phials with nitrous air, and covering the mouths of them with my finger, placed them inverted in a basin of the acid, when the absorption would

would instantly commence, and the fluid, without any agitation, would rise gradually and visibly, till the greatest part of the air disappeared. Making the experiments in this manner, I observed that the upper part of the acid, on which the nitrous air rested, became first of a deep orange, and then of a green colour.

In order to observe the full effect of nitrous air on a given quantity of strong nitrous acid, I filled a small phial with it, and then introduced it through the water into a large jar previously filled with nitrous air, and supported the phial in such a manner, as that the water could never rise so high as to get into it. In these circumstances, the surface of the liquor, which was at first of a pale yellow, presently assumed a deep orange colour, and the quantity of air absorbed was indeed very great. I was so much struck with this experiment, that I repeated it very often; and the following is a distinct recital of all the remarkable appearances attending one of them, which I select from the rest, as I noted them more minutely than in any other process of the kind.

Having filled a phial, containing exactly the quantity of four pennyweights of water, with a strong pale yellow spirit of nitre, with its mouth quite close to the top of a pretty large receiver, standing in water, I carefully drew out almost all

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the common air, and then filled it with nitrous air; and as this was absorbed, I kept putting in more, till, in less than two days, it had completely absorbed 130 ounce measures.

Presently after this process began, the surface of the acid assumed a deep orange colour, and when twenty or thirty ounce measures of air were absorbed, it began to be sensibly green at the top; and this green kept descending lower and lower, till it reached the bottom of the phial. Towards the end of the process, the evaporation of the acid was perceived to be very great; and when I took it out, the quantity was found to have been diminished exactly one half: for there remained no more than the quantity of two pennyweights of water. Also it had become, by means of this process and the evaporation together, exceedingly weak, and was rather *blue* than green.

The phial of spirit of nitre in this experiment was supported by an iron wire, rising from a flat piece of brass; and having at one time filled the receiver quite full of nitrous air, so as to leave the whole stand quite bare, I observed that great quantities of air issued from them, the moisture on their surface rendering this effect very apparent. This must have been an additional quantity of nitrous air, produced by the nitrous vapour which had been exhaled from the phial, or deposited in
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the decomposition of the nitrous air, and must be considered as having been absorbed by the acid in the phial, besides that which I threw into it. How much was the amount of this additional quantity of nitrous air, decomposed by the acid in the phial, I cannot certainly tell; but should guess, from the circumstances, that it could not be less than twenty ounce measures; which, added to the 130 above-mentioned, makes the whole quantity absorbed to have been 150 ounce measures. Besides, I withdrew the phial before the absorption had quite ceased.

At another time I was determined that the nitrous acid should continue in nitrous air till it could not possibly absorb any more of it, in order to observe what the *acid itself* would become after being fully saturated with phlogiston in this manner, and when it had at the same time exhaled as much as it could of its own acid in those circumstances. The consequence was, that, in four or five days, when the process terminated, the acid was become of a very light blue colour, and, as in the former case, was reduced to half its dimensions; so that the evaporation of the acid in this confined situation ceases before it becomes quite transparent, as it does by long exposure to the open air, though it is very possible, that a much longer continuance,

even in these circumstances, would have the same effect.

The above-mentioned experiments were made with the strongest yellow spirit of nitre. When I exposed to the nitrous air a quantity of *blue* spirit of nitre, the air was absorbed, but by no means in so great a quantity as by the other acid; and the surface of this acid became of a deeper blue in these circumstances. Had it been continued longer, it would, I suppose, have returned to a lighter blue by the evaporation of its acid; in which state it would have lost its power of attracting phlogiston from the nitrous air, as in the last-mentioned experiment. The nitrous air which had been exposed to this blue spirit of nitre was diminished a little by fresh nitrous air.

Having observed the change that took place in nitrous air by means of spirit of nitre, I was desirous of knowing whether the spirit of nitre in which it was agitated *acquired* or *lost* strength; when I soon found that, in consequence of getting more phlogiston, its power of dissolving metals was diminished, though it will be seen that the acid must have been weakened a little in the course of the experiment.

In order to effect my purpose, I first filled a phial with the nitrous acid, which was very strong,
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and of a pale yellow colour; and placing it inverted in a bason of the same, I introduced to it, by means of a bladder, a quantity of nitrous air; and when the acid had absorbed as much as it could of this, I threw out the residuum of phlogisticated air, and filling it up again with spirit of nitre from the same bason, I supplied it with more nitrous air.

This I continued to do for a considerable time, and observed that by the process the acid became very brown, and smoking; in consequence, no doubt, of having acquired phlogiston from the nitrous air. In dissolving copper with this acid, immediately after the process, I found that it was become weaker in the proportion of five and a half to seven. It must be observed, however, that the evaporation during the process (though I made it as expeditiously as I possibly could) must have weakened the acid a little, and also the end of a wet glass tube (though I never failed to wipe it as well as I could) being dipped into it every time that I supplied it with more air, must have diluted it a little more.

Observing the readiness with which nitrous acid decomposed nitrous air, by depriving it of its phlogiston, I had the curiosity to try how far the agitation of a quantity of this air in strong spirit of nitre would depurate it; and it was not without

surprize that I found that, when this process had continued but a very short time, the air had become so far pure by the loss of its phlogiston, that two measures of it and one of fresh nitrous air occupied the space of two measures and two thirds. I then tried the effect of this process on air phlogisticated by nitrous air, and found that this also was considerably improved by this means.

In both these cases the air was far from being so pure as to be fit for respiration; but that any kind of air should be reduced by this process to a state that is at all *better* than perfectly phlogisticated, will appear extraordinary, when it is considered, that, notwithstanding the affinity there is between this acid and phlogiston, yet that the vapour of it never fails to impart phlogiston to common air, so as to deprave it considerably. In several cases I have observed that common air thus exposed to the influence of nitrous vapour has become perfectly phlogisticated in a very short space of time. It should seem that the nitrous acid, when combined with water, has a stronger affinity with phlogiston than it retains in the form of vapour, free from water.

The effect of oil of vitriol, and spirit of salt, on nitrous air is by no means so remarkable as the effect of the nitrous acid upon it; but it is sufficiently

sufficiently evident that both these mineral acids do really decompose this air in part; and the impregnation they receive from the phlogiston they take from it is worth notice.

Oil of vitriol imbibes almost as much nitrous air as water can do, and requires about the same degree of agitation, or rather more, to effect it. Two thirds of the quantity of the air admitted to about four times as much of the acid was imbibed, and the oil of vitriol, which was before quite colourless, assumed a beautiful purple hue.

Spirit of salt imbibes nitrous air very slowly, and in a small quantity; but by this small impregnation, from being of a light straw colour, it became of a beautiful sky blue, very visible when held up to the light. The quantity absorbed was about one twentieth of its own bulk, and one third of the nitrous air employed in the experiment. In order to observe what proportion of nitrous air a quantity of spirit of salt would absorb with *long standing*, I suffered them to continue in contact in one case about two months; and after that time about two thirds of the air, which was originally about one fourth of the bulk of acid, was imbibed; and I imagine that with more time, still more of the air will disappear.

Nitrous air was readily absorbed without agitation by water impregnated both with *vitriolic acid*

air and *fluor acid air*. Each took more than its bulk, and not more than one twentieth part of the nitrous air remained unabsorbed. How much more would have been absorbed I did not try. No change of colour was produced by the process. N. B. Agitation only set loose the vapour of these acid liquors, and thereby increased the apparent bulk of the air. These two kinds of acid air imbibing nitrous air in the same manner is an argument for their being ultimately the same thing.

Both *radical vinegar*, and *concentrated vegetable acid*, absorbed nitrous air considerably faster than water. Of these acid liquors the former retained its transparency; for though, during the agitation it suddenly became of a turbid white, that change took place on the accidental admission of a bubble or two of common air, though I do not understand how this circumstance could produce that effect. The *concentrated vegetable acid* assumed a dark purple in consequence of this impregnation, very much resembling the oil of vitriol after the same process.

SECTION V.

Of the antiseptic Power of nitrous Air.

IT will perhaps be thought, that the most *useful*, if not the most remarkable, of all the properties of this extraordinary kind of air, is its power of preserving animal substances from putrefaction, and of restoring those that are already putrid, which it possesses in a far greater degree than fixed air. My first observation of this was altogether casual. Having found nitrous air to suffer so great a diminution by a mixture of iron filings and brimstone, I was willing to try whether it would be equally diminished by other causes of the diminution of common air, especially by putrefaction; and for this purpose I put a dead mouse into a quantity of it, and placed it near the fire, where the tendency to putrefaction was very great. In this case there was a considerable diminution, viz. from five and a quarter to three and a quarter; but not so great as I had expected, the antiseptic power of the nitrous air having checked the tendency to putrefaction; for when, after a week, I

took the mouse out, I perceived, to my very great surprize, that it had no offensive smell.

Upon this I took two other mice, one of them just killed, and the other soft and putrid, and put them both into the same jar of nitrous air, standing in the usual temperature of the weather, in the months of July and August of 1772; and after twenty five days, having observed that there was little or no change in the quantity of the air, I took the mice out; and, examining them, found them both perfectly sweet, even when cut through in several places. That which had been put into the air when just dead was quite firm; and the flesh of the other, which had been putrid and soft, was still soft, but perfectly sweet.

In order to compare the antiseptic power of this kind of air with that of fixed air, I examined a mouse which I had inclosed in a phial full of fixed air, as pure as I could make it, and which I had corked very close. But upon opening this phial in water about a month after, I perceived that a large quantity of putrid effluvium had been generated; for it rushed with violence out of the phial; and the smell that came from it, the moment the cork was taken out, was insufferably offensive. Indeed Dr. Macbride says, that he could only restore very thin pieces of putrid flesh by means of fixed air.

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I once thought that if a little pains were taken with this subject, this remarkable antiseptic power of nitrous air might possibly be applied to various uses, perhaps to the preservation of the more delicate birds, fishes, fruits, &c. mixing it in different proportions with common or fixed air, and especially that anatomists might perhaps avail themselves of it; but Mr. Hey, who made the trial, found that, after some months, various animal substances were shriveled, and did not preserve their natural forms in this kind of air.

I have made a few experiments, in order to ascertain whether it be possible to derive any advantage from this property of nitrous air for *culinary purposes*. But I cannot say that my observations have been very favourable to it in this respect. Nitrous air will, indeed, preserve flesh meat from putrefaction; but after long keeping in this manner it becomes very offensive, both to the nostrils, and the palate, though the smell is not altogether that of putrefaction; and indeed the substance continuing quite firm, it could not be properly putrid. Though these experiments were not quite fair, because the nitrous air had not been renewed so often as it ought to have been, several of the phenomena may be worth mentioning.

On the 28th of April 1777, I put two pigeons into two jars of nitrous air, just wide enough to contain

contain them, with about as much nitrous air in the jars, as the bulk of the pigeons. From this time till the 4th of June following, I had renewed the nitrous air but once, and then, taking them out, I found them both free from all smell of putrefaction. One of them was broiled, when the flesh was found to be sweet, but it had not the natural taste of the pigeon, and was, on the whole, unpleasant. The flesh was quite red throughout, and a little harder than that of a pigeon generally is. The water contained in the cups, in which the jars with the pigeons had stood, had generally been very offensive, so that it should seem that the putrid effluvia (containing, probably, much phlogiston, and perhaps the most nutritive part of the flesh) had passed through the nitrous air, and the water, into the surrounding atmosphere.

I replaced the pigeon that was not used, and let it remain, along with two others which had been kept the same time, till the 13th of September following, in all, near six months, or the whole summer season; but I had not been careful to change the air very often, though I did it two days before I took them out the last time. The pigeons had now certainly a very bad smell, though their flesh was firm, and so were even the bowels of one of them which had not been drawn. When they were dressed, they were much more offensive, and had a
strong

strong smell of putrefaction, or something very much resembling it. The flesh was red throughout, still firm, and exclusive of the smell, had little or no taste. My friend, Mr. Magellan, who was with me at the preparation of them, had not so bad an opinion of this piece of cookery as I had.

On the 10th of May I put into a jar of nitrous air a large wood pigeon; and taking it out on the 18th of June following, observed that it had a strong and offensive smell, but the flesh was perfectly firm. Though a very great part of the air had been absorbed, and during the fortnight preceding the examination it had not been supplied with fresh air, as it had been occasionally before, the air to which it had been exposed all that time diminished common air quite as much as fresh made nitrous air. It was this observation that gave me the first suspicion of the manner in which nitrous air is diminished in this and in other processes. Having replaced the pigeon in the jar, I found on the 7th of August following, that the air was but slightly nitrous, and on the 22d of the same month it was mere phlogisticated air. After this I neglected to attend to it, and at last threw it away. Whether, in this process, the nitrous air ever comes into a state in which a candle will burn in it, or not, I cannot tell. The experiment is a very unpleasant one, and I shall hardly repeat it.

In all these cases the flesh was kept a long time, viz. through the six summer months ; and though nitrous air failed to preserve meat in a state fit for eating so very long, it may possibly answer the purpose for a few days tolerably well, as it will certainly restore meat that has begun to turn putrid. One trial of this kind I did make.

On the 14th of June 1777, I took a fowl which had been killed a week, and which had been purposely kept till it was offensive ; and putting it into a jar of nitrous air, observed that the air began immediately to be absorbed, and on the 16th I took the fowl out, when it had no smell of putrefaction at all ; but when it was boiled, though myself and several other persons tasted of it, and perceived nothing disagreeable in the taste itself, we were disgusted with a faint smell that came from the body of the fowl, when we held it to our nostrils. Perhaps it had not been exposed to the nitrous air quite long enough.

Though part of this air had been absorbed, the remainder diminished common air quite as much as any fresh made nitrous air.

On the subject of this section I shall observe that Dr. Millman having been so obliging as to inform me that he had found that *bile* is prevented from becoming putrid much longer by being impregnated with fixed air, than it could otherwise be ; I was desirous

desirous of trying what effect the impregnation with nitrous air would have upon it. Accordingly, on the 19th of February 1777, I impregnated a quantity of ox bile, with which he supplied me with nitrous air; when, from being viscid, it presently became limpid like water, and assumed a brownish hue, without depositing any thing that I could perceive. This bile continued perfectly sweet till the 20th of March following, when it was packed up, along with other things, and removed from London into the country. Examining it some time afterwards, I found it had contracted a smell of putrefaction, and on the 23d of April, it was quite putrid. The same brown colour continued, but it had deposited something of a whitish colour.

SECTION VI.

Of the Formation of nitrous Ammoniac by nitrous Air.

IN the mixture of this kind of air with common air, in a trough of water which had been putrid, but which at that time seemed to have recovered its former sweetness (for it was not in the least degree offensive to the smell) a phenomenon sometimes occurred, which for a long time exceedingly delighted and puzzled me.

When the diminution of the air was nearly completed, the vessel in which the mixture was made began to be filled with the most beautiful *white fumes*, exactly resembling the precipitation of some white substance in a transparent menstruum, or the falling of very fine snow; except that it was much thicker below than above, as indeed is the case in all chemical precipitations. This appearance continued two or three minutes.

Afterwards, having (with a view to observe whether any crystals would be formed by the union of volatile alkali, and nitrous air, similar to those formed by it and fixed air, as described by Mr. Smeth in his *Dissertation on fixed Air*) opened the mouth
of

of a phial which was half filled with a volatile alkaline liquor, in a jar of nitrous air, I had an appearance which perfectly explained the preceding. All that part of the phial which was above the liquor, and which contained common air, was filled with beautiful *white clouds*, as if some fine white powder had been instantly thrown into it, and some of these clouds rose within the jar of nitrous air. This appearance continued about a minute, and then intirely disappeared, the air becoming transparent.

Withdrawing the phial, and exposing it to the common air, it there also became turbid, and soon after the transparency returned. Introducing it again into the nitrous air, the clouds appeared as before. In this manner the white fumes and transparency succeeded each other alternately, as often as I chose to repeat the experiment, and would, no doubt, have continued till the air in the jar had been thoroughly diluted with common air. These appearances were the same with any substance that contained *volatile alkali*, fluid or solid.

When, instead of the small phial, I used a large and tall glass jar, this appearance was truly fine and striking, especially when the water in the trough was very transparent. For I had only to put the smallest drop of a volatile alkaline liquor, or the smallest bit of the solid salt, into the jar, and the moment that the mouth of it was opened in a jar of nitrous
air,

air, the white clouds above mentioned began to be formed at the mouth, and presently descended to the bottom, so as to fill the whole, were it ever so large, as with fine snow.

In considering this experiment, I soon perceived that this curious appearance must have been occasioned by the mixture of the nitrous and common air, and therefore that the white clouds must be *nitrous ammoniac*, formed by the acid of the nitrous air, set loose in the decomposition of it by common air, while the phlogiston, which must be another constituent part of nitrous air, entering the common air, is the cause of the diminution it suffers in this process; as it is the cause of a similar diminution, in a variety of other processes.

In diversifying this experiment, I found that it appeared to very great advantage when I suspended a piece of volatile salt in the common air, previous to the admission of nitrous air to it, inclosing it in a bit of gauze, muslin, or a small net of wire. For, presently after the redness of the mixture begins to go off, the white cloud, like snow, begins to descend from the salt, as if a white powder was shaken out of the bag that contains it. This white cloud presently fills the whole vessel, and the appearance will last about five minutes.

If the salt be not put to the mixture of these two kinds of air till it has perfectly recovered its transparency,

parency, the effervescence being completely over, no white cloud will be formed; and, what is rather more remarkable, there is nothing of this appearance when the salt is put into the nitrous air itself. The reason of this must be, that till common air be admitted to the nitrous, no acid is formed, to unite with the alkali, and make the nitrous ammoniac.

Having generally fastened the small bag which contained the volatile salt to a piece of brass wire in the preceding experiment, I commonly found the end of it corroded, and covered with a blue substance. Also the salt itself, and sometimes the bag was dyed blue. But finding that this was not the case when I used an iron wire in the same circumstances, but that it became *red*, I was satisfied that both the metals had been dissolved by the volatile alkali, or the acid. At first I had a suspicion that the blue might have come from the copper, out of which the nitrous air had been made. But when the nitrous air was made from iron, the appearances were, in all respects the same.

SECTION VII.

Explanation of some Phenomena attending the Solution of Metals in nitrous Acid.

AS the discovery of fixed air in calcareous substances threw new light upon many phenomena in chemistry, in like manner the discovery of every other kind of air, and indeed of every property of any of them, must throw light upon those processes in which they are concerned. Not being a professed chemist, and attending only to such articles in that branch of knowledge as my own pursuits are particularly connected with (though these necessarily grow more various and extensive continually) such illustrations of chemical processes are not so likely to occur to me, as they are to others, who by their profession give a general attention to every thing within the whole compass of chemistry. Such, however, as I have had occasion to attend to, and which I imagine I can throw any light upon, I shall not fail to mention.

There

There are many facts relating to the solution of metals in spirit of nitre, which could not have been understood without the knowledge of nitrous air; and yet, though several of them are very remarkable, I do not find that even the phenomena themselves, and much less the difficulties attending the solution of them, have been so much as noticed. I am persuaded, however, that an attention to the nature of this remarkable kind of air will contribute greatly to the investigation of the constitution of the several metals, and the explanation of many phenomena attending their decomposition, and consequently their composition.

Having had frequent occasion to dissolve mercury in strong spirit of nitre, in order to procure from it nitrous and dephlogisticated air, and to note the quantity of the metal revived afterwards, I could not help being very particularly struck with some phenomena in the solution, which are as follows.

The moment that strong spirit of nitre is poured upon quicksilver, the solution is instantly very rapid. But though it is known that one method of procuring nitrous air is by the solution of this metal in the nitrous acid, not a single bubble of any kind of air is seen to be formed; at least none rises through the acid. Presently, however, one may perceive, that

very large bubbles of air *are* formed, but they instantly disappear, and nothing remains of them but the smallest specks imaginable, to rise to the top of the acid. By degrees, the acid near the mercury becomes of a deep orange colour, and then through this part of the acid the bubbles of air ascend freely; but the moment they come to the superincumbent pale coloured acid, they collapse into those small and barely perceivable points, yielding no air that can be collected in any sensible quantity. And it is not till the whole quantity of the acid is changed from a pale to an orange colour, that any nitrous air can be collected. Then, however, the bubbles rise freely to the top of the acid, and, mixing with the incumbent common air, exhibit an orange colour by their decomposition on mixing with it. Then, also, a strong smell of spirit of nitre is perceived, as it always happens when nitrous air is let loose to mix with the air of the room in which we are breathing. Whereas, immediately before, no smell was perceived, and the common air incumbent on the mixture was quite colourless.

Had these singular phenomena been noticed by any chemist before the discovery of nitrous air, I cannot imagine what hypothesis he would have formed for the explanation of them. Whatever it had been, it must have been very wide of the truth; whereas

whereas the whole process admits of the easiest explanation imaginable by the help of my observations on the decomposition of nitrous air by the nitrous acid.

Nitrous air is actually formed the moment that the solution begins, but it is instantly decomposed by the strong spirit of nitre in contact with it. By the addition of the phlogiston contained in the nitrous air, the pale spirit of nitre assumes an orange colour, and it is then much less able to decompose the nitrous air; which, therefore, rises in bubbles through it, and is not decomposed till it comes to the region of the pale acid lying upon it. But when the whole body of the acid is saturated with phlogiston, then, and not before, the bubbles of nitrous air pass freely through it, and may be collected.

On this account, it is not easy to ascertain the exact quantity of nitrous air yielded by the solution of mercury, and, for the same reason, of other metals too, in strong spirit of nitre; because allowance must be made for the quantity that will be imbibed by the acid itself, which must be saturated before any can be collected; whereas, when the acid is much diluted with water, it is not so capable of decomposing this air, and therefore, in general, it may be collected from the moment that the solution begins.

It

It is very remarkable, that when copper is dissolved in pale spirit of nitre, even diluted with much water, though the solution is evidently the most rapid at the first, the produce of air is very trifling for a considerable time, and the quantity collected increases very gradually; whereas when the orange coloured acid is employed, in the same diluted state, the nitrous air is collected immediately, and the production is the most copious at the first.

When I dissolved a quantity of copper in strong spirit of nitre half diluted with water, no air whatever was produced, though the metal was completely dissolved.

When, in the solution of mercury, I used the green spirit of nitre, instead of the pale coloured and strongest acid, the phenomena were not materially different from those described above. The lower part of the acid next to the mercury assumed a deeper green, but it never became orange coloured.

SECTION VIII.

Miscellaneous Properties of nitrous Air.

1. *Of the freezing of Water impregnated with nitrous Air.*

I HAVE observed, that water discharges all the fixed air it had imbibed the moment that it is converted into ice. The same is the case with water impregnated with nitrous air, as appears by the following experiment, made with a view both to this circumstance, and also to the earthy precipitate deposited by water thus impregnated.

Having impregnated a quantity of water with nitrous air, I exposed it to the frost, and observed that it did not freeze quite so soon as a quantity of the same water which had not been so impregnated, exposed in the same manner. The ice of the impregnated water was full of very small bubbles, and when it was thawed did not turn the juice of turnsole red in the smallest degree. It also made a consider-

able precipitate of a very white matter, exactly like that which I procured from the water impregnated with nitrous air from bismuth. This nitrous air, however, had been procured from copper.

Having exposed to the frost a quantity of water which had been a long time before impregnated with nitrous air, and which had spontaneously deposited a brownish sediment, it now deposited more of the same colour.

2. *Of the burning of a Mixture of nitrous and inflammable Air.*

Inflammable air with a mixture of nitrous air burns with a green flame. This makes a very pleasing experiment when it is properly conducted. As, for some time, I chiefly made use of copper for the generation of nitrous air, I first ascribed this circumstance to that property of this metal, by which it burns with a green flame; but I was presently satisfied that it must arise from the spirit of nitre, for the effect is the very same from which ever of the metals the nitrous air is extracted, all of which I tried for this purpose, even silver and gold. When a candle is extinguished, as it never fails to be, in nitrous air, the flame seems to be a little

little enlarged at its edges, by another bluish flame added to it just before its extinction.

3. *Of Plants and Animals in nitrous Air.*

Plants die very soon, both in nitrous air, and also in common air saturated with nitrous air, but especially in the former.

This kind of air is as noxious as any whatever, a mouse dying the moment it is put into it; but frogs and snails (and therefore, probably, other animals whose respiration is not frequent) will bear being exposed to it a considerable time, though they die at length. A frog put into nitrous air struggled much for two or three minutes, and moved now and then for a quarter of an hour, after which it was taken out, but did not recover.

There is something remarkable in the effect of nitrous air on *insects* that are put into it. *Wasps* always died the moment they were put into the nitrous air. I could never observe that they made the least motion in it, nor could they be recovered to life afterwards. This was also the case in general with *spiders*, *flies*, and *butterflies*. Sometimes, however, spiders would recover after being exposed about a minute to this kind of air.

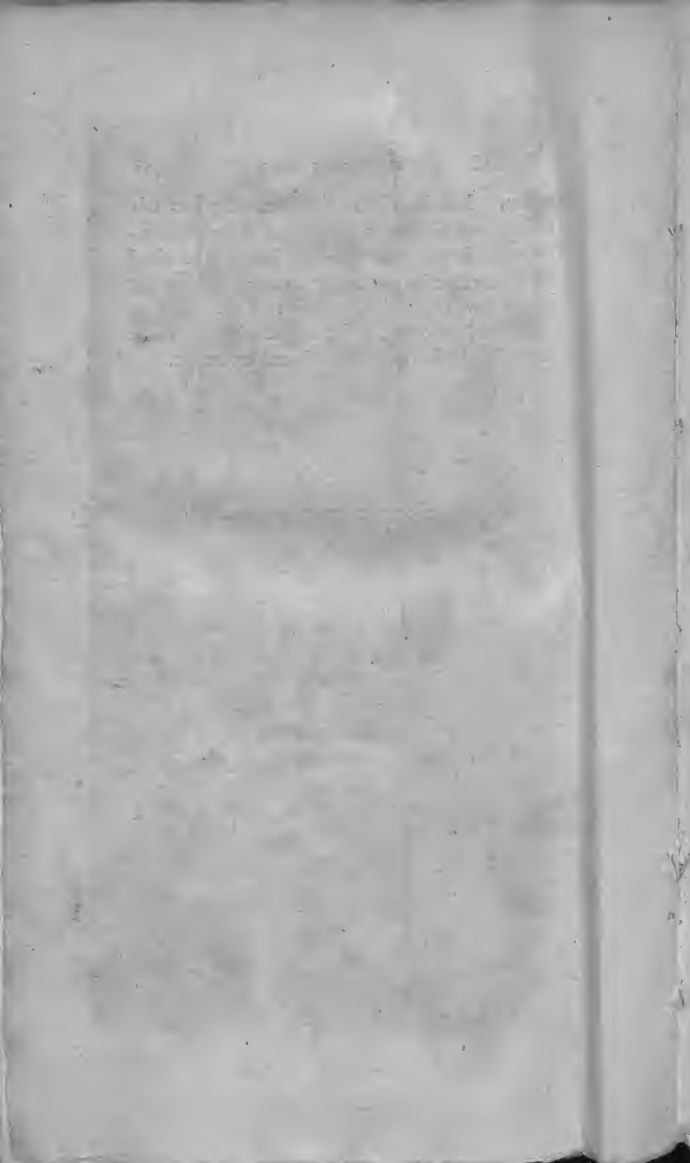
4. *Of the Use of nitrous Air in Clysters.*

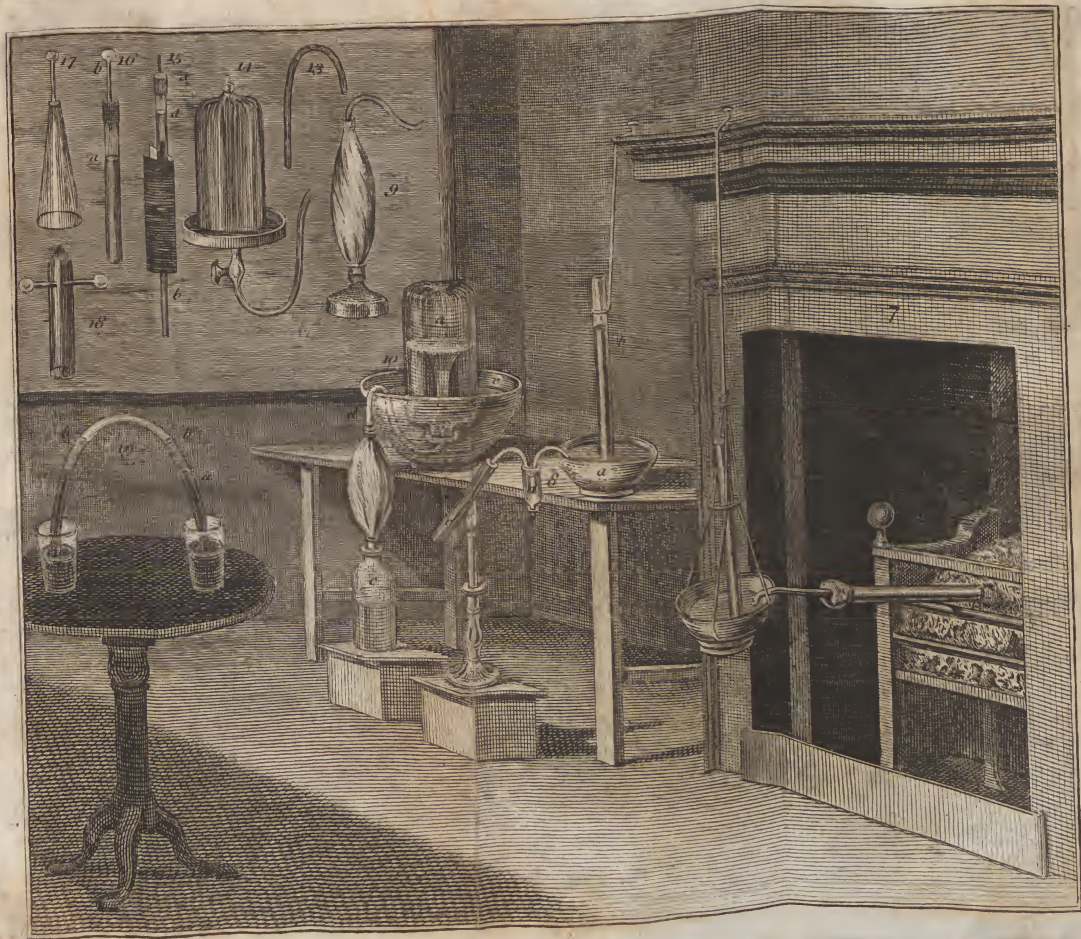
Considering how fatal nitrous air is to insects, and likewise its great antiseptic power, I conceived that considerable use might be made of it in medicine, especially in the form of *clysters*, in which fixed air had been applied with some success; and in order to try whether the bowels of an animal would bear the injection of it, I contrived, with the help of Mr. Hey, to convey a quantity of it up the anus of a dog. But he gave manifest signs of uneasiness as long as he retained it, which was a considerable time, though in a few hours afterwards he was as lively as ever, and seemed to have suffered nothing from the operation.

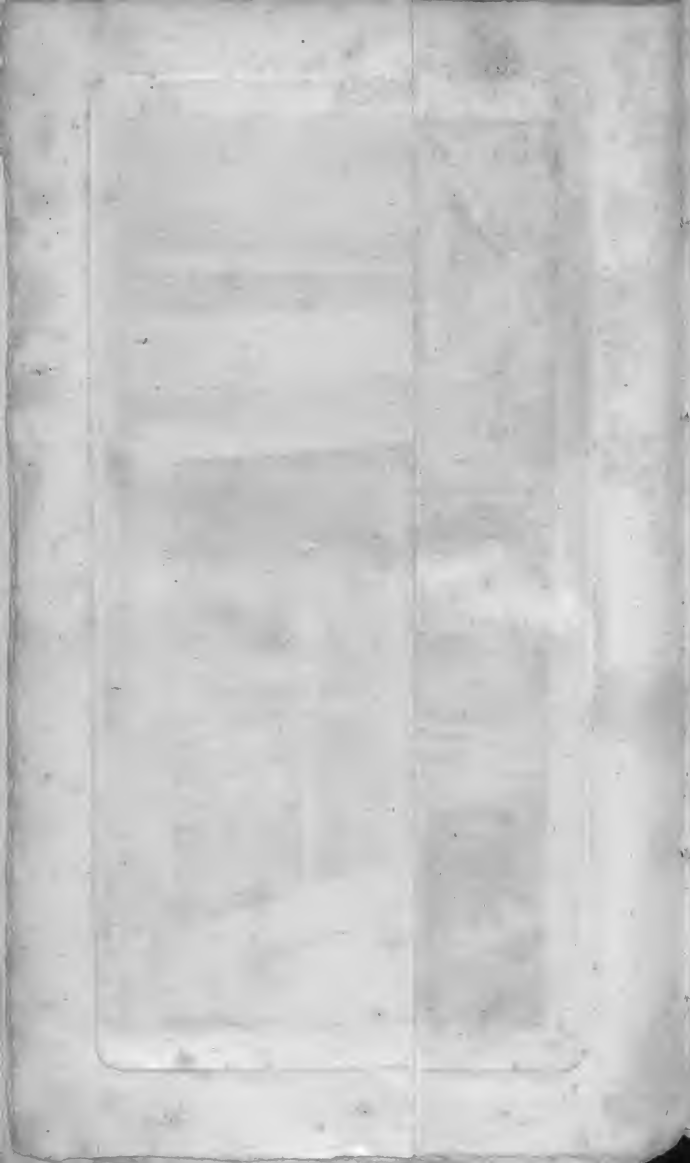
Perhaps if nitrous air was diluted either with common air, or fixed air, the bowels might bear it better, and still it might be destructive to *worms* of all kinds, and be of use to check, or correct, putrefaction in the intestinal canal, or other parts of the system. I repeat it once more, that, being no physician, I run no risk by such proposals as these; and I cannot help flattering myself that, in time, very great medicinal use will be made of the application of these different kinds of air to the animal system.

system. Let ingenious physicians attend to this subject, and endeavour to lay hold of the new *bundle* which is now presented them, before it be seized by rash empirics; who, by an indiscriminate and injudicious application, often ruin the credit of things and processes, which might otherwise make an useful addition to the *materia* and *ars medica*.

END OF THE FIRST VOLUME.







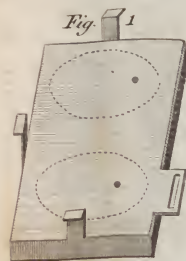








Fig.1.



Fig.2.



Fig.3.

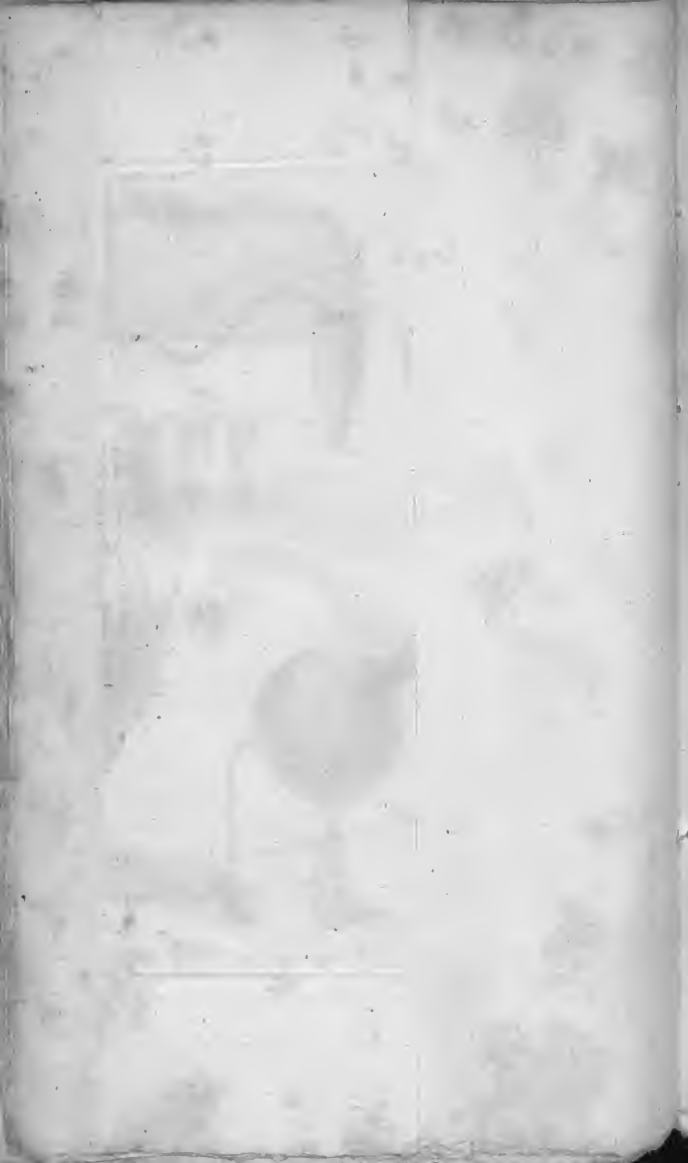


Fig.4.



Fig.5.





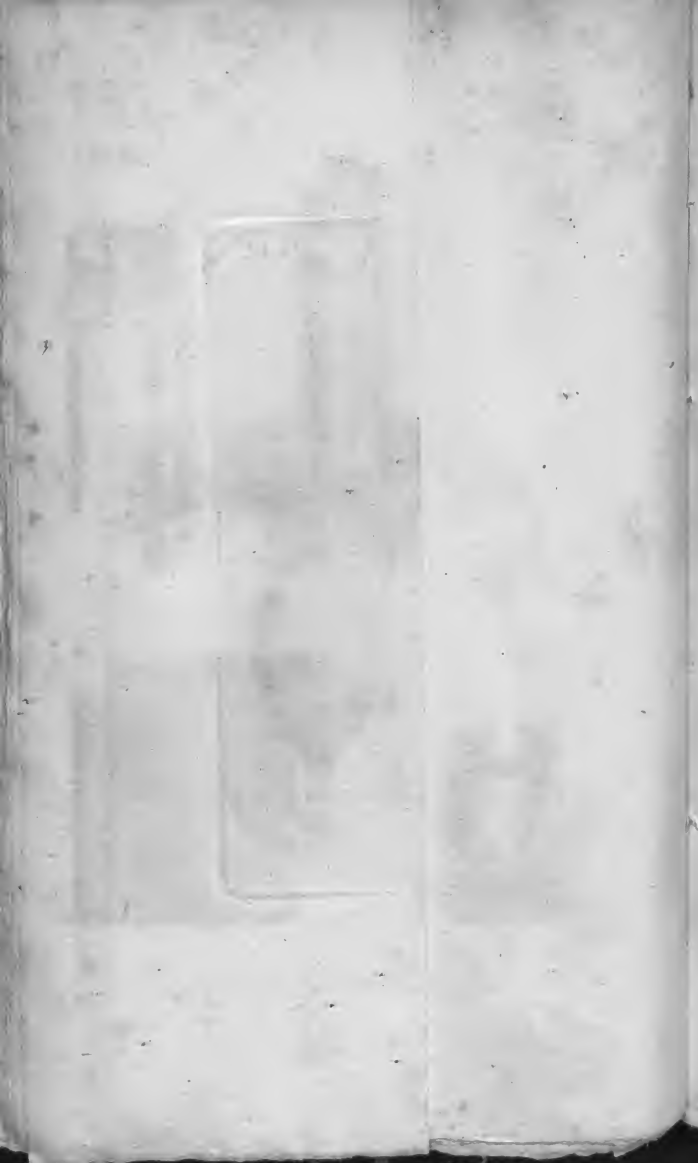
Pl. VI.

Fig. 1.



Fig. 2.





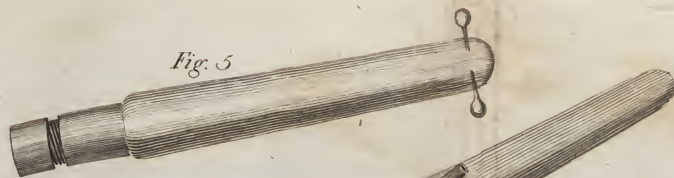
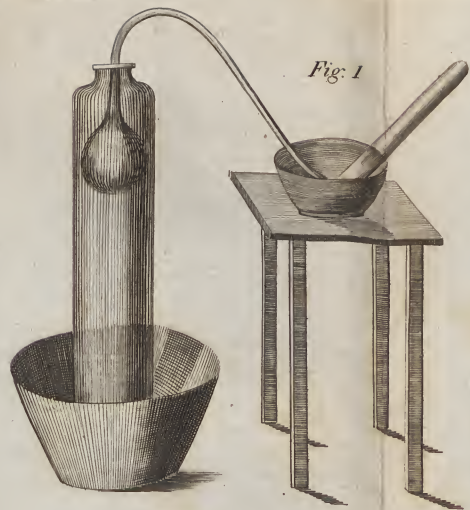


Fig. 4.



Fig. 3

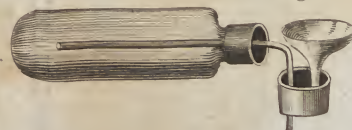


Fig. 6

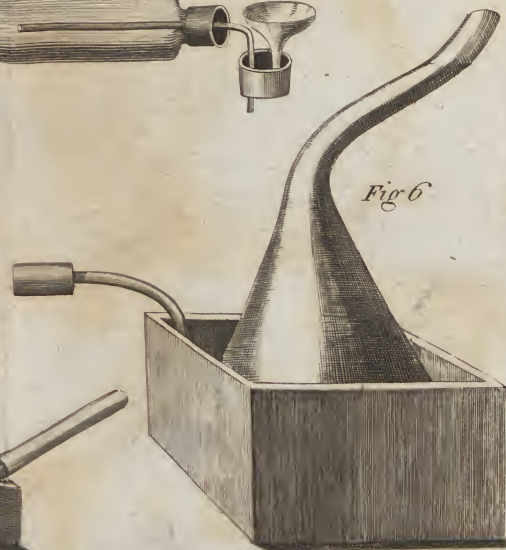
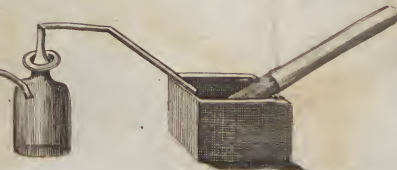
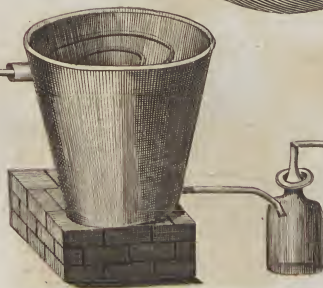
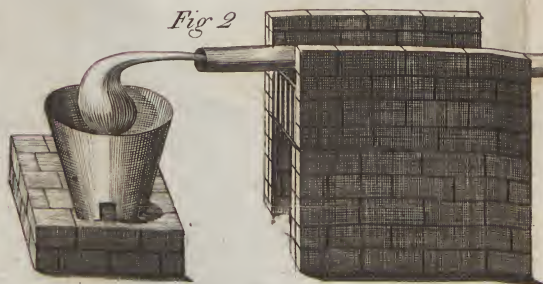


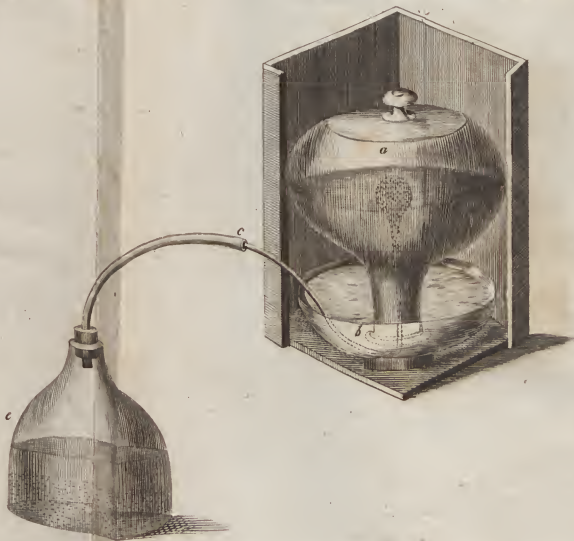
Fig. 2



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Pl. VIII



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